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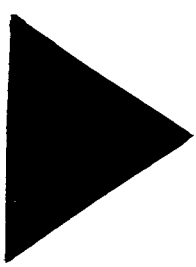
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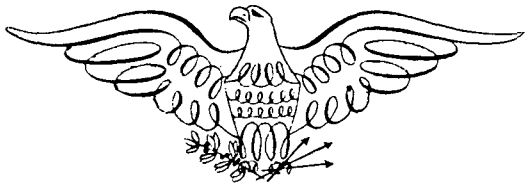


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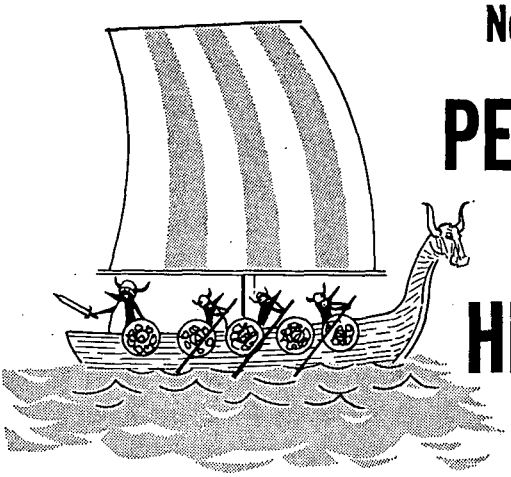
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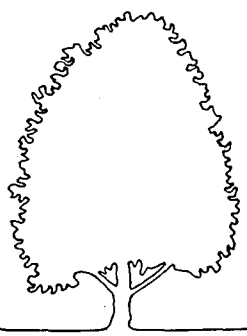
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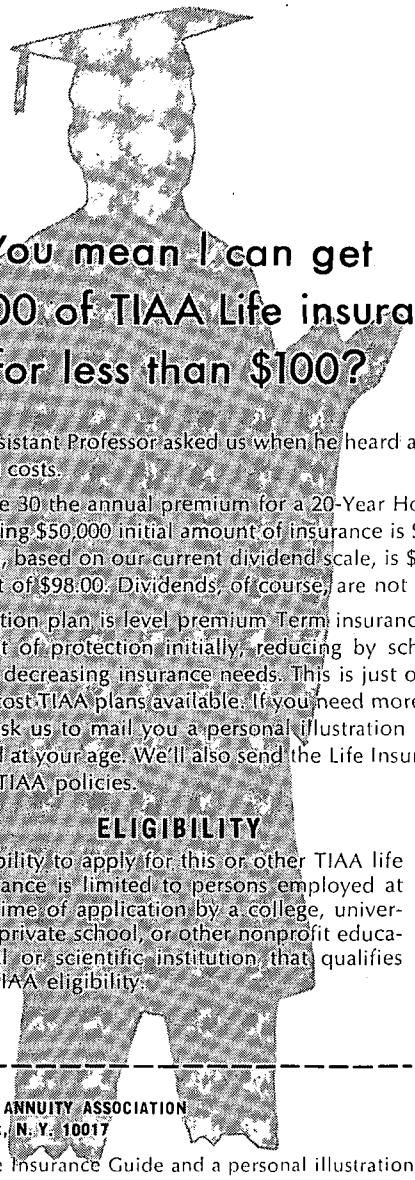
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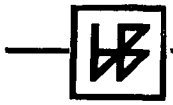
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The Goals of Italian Fascism

EDWARD R. TANNENBAUM

THE purpose of this essay is to discover what the Italian Fascists *wanted,* not what they did or who supported them. Any explanation of their rise to power would have to include the nature of the Italian crisis during and immediately after the First World War, the ways in which the ruling circles and opposition parties abdicated their responsibilities in the face of this crisis, and, finally, Mussolini's tactical ability.[1] Most people who accepted Mussolini at first were interested in the restoration of law and order, not in squadrism, syndicalism, or demagogic nationalism. By the mid-1920's his re-

► *Mr. Tannenbaum, a professor at New York University, is the author of* The New France *(Chicago, 1961). In 1950 he received his Ph.D. from the University of Chicago where he worked with S. William Halperin and Louis Gottschalk. This article, which was supported by a grant from the American Philosophical Society, is based on material from the author's forthcoming book, "The Fascist Experience: Italian Society and Culture, 1922–1945."*

[1] The literature on the rise of Fascism is vast. Some of the most important recent studies are: Costanzo Casucci, *Il fascismo: Antologia di scritti critici* (Bologna, 1961); Paolo Alatri, *Le origini del fascismo* (Rome, 1961); Angelo Tasca, *Nascita e avvento del fascismo: L'Italia dal 1918 al 1922* (Florence, 1963); Antonio Repaci, *La marcia su Roma: Mito e realtà* (2 vols., Rome, 1963); *Il fascismo e i partiti italiani: Testimonianze del 1921–1923,* ed. Renzo De Felice (Bologna, 1966); Roberto Vivarelli, *Il dopoguerra in Italia e l'avvento del fascismo (1918–1922)* (Naples, 1967). Renzo De Felice's *Mussolini il rivoluzionario* (Turin, 1965) and *Mussolini il fascista* (2 vols., Turin, 1966–68) are also very useful. For other titles, see Emiliana P. Noether, "Italy Reviews Its Fascist Past: A Bibliographical Essay," *American Historical Review,* LXI (July 1956), 877–99; and Charles F. Delzell, "Benito Mussolini: A Guide to the Biographical Literature," *Journal of Modern History,* XXXV (Dec. 1963), 339–53. Several comparative studies on the rise of Fascism illuminate the situation in Italy: Élie Halévy, *The Era of Tyrannies,* tr. R. K. Webb (New York, 1965); Ernst Nolte, *Die faschistischen Bewegung* (Munich, 1966); Francis L. Carsten, *The Rise of Fascism* (London, 1967); *European Fascism,* ed. Stuart Woolf (London, 1968).

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gime adopted much of the authoritarian program of the prewar Italian Nationalist Association, with which the Fascist party had merged in 1923.[2] Although each of these two movements already had its own internal divisions, their fusion engendered the most acrid arguments over ideology between the half-educated former *squadristi* leaders who had made the "Revolution" of 1919–1922 and the nationalist intellectuals who tried to use this "Revolution" for their own ends thereafter. There was also a conflict of generations here: aside from Mussolini himself most Fascist ras were still in their twenties when he gained power, and they resented the apparently preponderant influence of the older nationalists, especially Luigi Federzoni and Alfredo Rocco, soon thereafter. The Fascist syndicalists had wanted a new, autonomous labor movement, not the absorption of the workers into corporations dominated by the employers and controlled by the state. The *squadristi* had wanted the triumph of rebellious youth over the existing order and the older generation in all the other parties, not a bureaucratic Fascist party machine stripped of its political power. For many of them Fascism continued to mean beating people with a big club (affectionately called the *santo manganello*) and performing daredevil feats of courage.[3]

The diverse goals of the Fascists have been obscured by the public behavior of their *Duce*. This is unfortunate, for the blackshirted *squadristi* were the first and most notorious examples of one recurring form of youthful revolt against self-righteous liberal establishments. (Today's Hell's Angels motorcycle gang, with its black leather jackets, typifies this kind of revolt better than the white-hooded Ku Klux Klan, which is temperamentally reactionary.) Like Hitler, Mussolini had first expressed his disgust for the prevailing order before the First World War, but whereas Hitler got his initial ideological orientation from reactionary anti-Semites like Georg von Schönerer, Mussolini claimed to have been a disciple of Georges Sorel. The importance of these "influences" should not be exaggerated, but the fact remains that Hitler had never been a man of the Left, and he never attracted as many Left-wing revolutionary followers as Mussolini did. Once in power Mussolini found it politically expedient to maintain much of the old order against which he had been advocating one form of revolution or another for over two decades. He certainly was opportunistic in his compromises with the monarchy, the state administration, the army, the big industrialists and landowners, and the

[2] See Franco Gaeta, *Nazionalismo italiano* (Naples, 1965).

[3] For the ways in which Mussolini disposed of recalcitrant former *squadristi* and subordinated the party to the state, see De Felice, *Mussolini il fascista;* and Alberto Aquarone, *L'Organizzazione dello stato totalitario* (Turin, 1965). Two earlier surveys that present information on these matters are Luigi Salvatorelli and Giovanni Mira, *Storia d'Italia nel periodo fascista* (5th ed., Turin, 1964), from an anti-Fascist point of view; and Attilio Tamaro, *Venti anni di storia, 1922–1943* (3 vols., Rome, 1952–54), from a pro-Fascist point of view.

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Church. Yet even he never lost his hostility toward these groups or his desire to downgrade them in some way. Nor was he the only dictator who sacrificed ideology to state policy.

In contrast to Nazism, Fascism, at least in the beginning, had tried to present itself as a revolutionary alternative to Marxism rather than a mindless reaction against it. The founders of the Fascist movement in March 1919 were almost all former revolutionary socialists and syndicalists who by late 1914 had organized Fasci for Revolutionary Action urging intervention in the war. Their 1919 program was anticapitalist, antimonarchical, anticlerical, antisocialist, antiparliamentary, and, most especially, antibourgeois; they wanted to overthrow the existing establishment and replace it with a rather vague national syndicalist utopia. At first Mussolini viewed himself as a kind of Italian Lenin, with a desire to continue the war with a revolution.

By 1921, however, the original character of the movement had changed radically. As the fruits of victory turned sour, and as the liberal regime seemed powerless to control profiteers and strikers, many inexperienced, alienated students and younger veterans developed a "lost generation" complex and joined the Fascists in order to sweep away the sordid "mess" around them and to build a purer, nobler order. Beginning in late 1920 the rural *squadristi* in the Po Valley gave the movement an increasingly violent stance. These young rowdies introduced a distinctly provincial and anarchic quality to Fascism, specializing in vicious "punitive raids" against socialist and Catholic labor unions and the headquarters of rival parties. Many *squadristi* leaders accepted subsidies from the big landowners and businessmen, but they did not abandon their ultimate goal of making their own revolution.

After the Fascist regime was established, party spokesmen continued to expound divergent views about its nature and goals. Aside from glorification of the *Duce* and the nation the only slogan that most of them seemed to accept was the "continuing revolution." The public was told that the refusal of the regime to be imprisoned by a particular idea was a sign of its "living force, that is to say its validity as truth."[4] But Fascist ideologues of both the Left and the Right interpreted the "continuing revolution" in several different ways. Edmondo Rossoni, president of the National Confederation of Fascist Unions during the mid-1920's, wanted Fascist syndicalism to be an effective champion of Italian labor against selfish employers. Roberto Farinacci, still firmly entrenched as the ras in Cremona after his dismissal as national party secretary in March 1926, continued to appeal for a resumption of squadrism. Giuseppe Bottai, Minister of Corporations and then Minister

[4] See, e.g., Balbino Giuliano, *Elementi di cultura fascista* (5th ed., Bologna, 1940), 3. Giuliano, a neoidealist philosopher and disciple of Giovanni Gentile, was the Minister of Education from 1929 to 1932.

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of Education in the 1930's, wanted new technical and cultural elites; he and his colleagues on the review *Critica fascista* urged the modernization of all aspects of Italian life. The reactionary nationalist Alfredo Rocco advocated, on the other hand, a constitutional corporate state.

It is obviously not to Mussolini, then, that we must turn for an understanding of Fascist goals but to those party ideologues who tried to push him in one direction or another. Until his dictatorship was firmly secure, he had considered the party as merely the military instrument for achieving his own personal power; thereafter he continued to view it with a mixture of suspicion and contempt.[5] It was natural that the revolutionary activists who had participated in the march on Rome should have wanted to base the new Fascist order on their mode of organized violence and on an omnipotent but decentralized party oligarchy. In order to counteract this tendency (as well as to placate the conservatives) Mussolini temporarily enlisted the services and ideas of those nationalist converts to Fascism who preached the primacy of the state and its law enforcement agencies. The victory of Federzoni as Minister of the Interior over Farinacci as party secretary by early 1926 is well known,[6] as are Mussolini's efforts to exploit the prestige of the philosopher Giovanni Gentile by adopting phrases like the "ethical state." Most important of all was the *Duce*'s decision to allow Rocco to create the constitutional façade of a "corporate state." Mussolini had no faith in trade-union leaders, technocrats, intellectuals, or any other kind of elite, yet he humored syndicalists like Pietro Capoferri[7] as well as champions of a "managerial revolution," especially the editors of *Critica fascista*. He continued to tolerate such heterodoxy not only because of his opportunism and his diffidence toward the party but also because he simply preferred to avoid open confrontations.

Me ne frego ("I don't give a damn") was one of the youthful Black Shirts' battle cries. Italo Balbo, the ras of Ferrara and one of the most glamorous *squadristi* leaders, said in early 1922 that the Socialist daily *Avanti!* rightly interpreted this slogan as an expression of "contempt for every norm of established government, irony and practical jokes in the face of all authority, the insolence of lawless musketeers." According to Balbo, the existing regime was "our battle objective. We want to destroy it with all of its venerated institutions. The greater the scandal generated by our actions the hap-

[5] De Felice, *Mussolini il fascista*, 427.

[6] In the interest of domestic harmony Federzoni wanted to remove the impression that the Ministry of the Interior had won out over the party. In order to achieve this purpose he suggested that he resign and that Mussolini himself take over the ministry. (See Federzoni to Mussolini, Apr. 16, 1926, Partito Nazionale Fascista, Segretaria Particolare del Duce, Carteggio Riservato [hereafter cited as Seg. Part. del Duce], fasc. 82/R, Archivio Centrale dello Stato.)

[7] Pietro Capoferri, *Venti anni col fascismo e con i sindacati* (Milan, 1957), 50–51. Capoferri was the head of the Fascist *sindacati* in the province of Milan throughout the 1930's and the president of the National Confederation of Industrial Workers after September 1939.

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pier we are."[8] As with self-styled revolutionary activists elsewhere, organized violence became a habit with many *squadristi.*

Not only did the *squadristi* not "give a damn" about their scandalous behavior; they were also not unduly concerned about who their victims were. Beginning on November 21, 1920, in a murderous battle at the town hall of Bologna, they set out to dislodge the Socialist and Communist politicians who had recently gained control of the local governments in Romagna, Emilia, and other regions of the Po Valley.[9] At about the same time and in the same area they leveled their sights at the leaders of the agricultural laborers' unions, attacking "Bolsheviks," anarchists, and Catholics indiscriminately. A year later Renato Ricci and his squads in Tuscany overthrew the republican mayor of Carrara.[10] Before the fusion with the Nationalists early in 1923 the *squadristi* had violent brawls with their "Blue Legions," especially in the South. In 1924 rival gangs of *squadristi* were fighting each other —in Bergamo, Novara, Milan, Bari, Naples, Messina, Livorno, Genoa, and Bologna.[11] These incidents of organized violence for its own sake created a serious crisis for Mussolini's new government, a crisis that was aggravated by the murder of the Socialist deputy Giacomo Matteotti, the most famous victim of *menefreghismo.*

While the rank and file of the *squadristi* were little more than patriotic young gangsters, their leaders wanted a revolution that would give them a monopoly of power; in order to achieve this goal they first had to defeat the local forces of the Left. Only after they already controlled a number of important local governments, at least in the North, could they hope to challenge the central government itself. Hence, in 1921–1922, their "Revolution" took the form of a series of local civil wars in which their victims were almost invariably the workers' organizations, both urban and rural.[12] Some union and Left-wing party leaders had been trying to make their own revolution since

[8] Italo Balbo, *Diario 1922* (Milan, 1932), 30, entry for Mar. 2.

[9] The Fascist "assault" on the Bologna town hall inspired the young Dino Grandi to found the weekly *L'Assalto,* which from its first issue on December 1, 1920, quickly became one of the most militant spokesmen for *squadristi* violence. Grandi, who had been influenced by the defrocked Left-wing Catholic priest Romolo Murri and who claimed to be a latter-day disciple of Mazzini, was soon to become one of the party's leading moderates, but he ended his first editorial in *L'Assalto* with the following ultimatum to the Socialists: "For the grandeur and well-being of all the people the Fascists today make their second declaration of war. Without an armistice and without quarter."

[10] Antonio Bernieri, "Il fascismo a Carrara tra il 1919 e il 1931," *Movimento operaio e socialista,* X (Jan.–Mar. 1964), 48.

[11] See Ministero dell'Interno, Direzione Generale, Pubblica Sicurezza, Divisione Affari Generali e Riservati, busta 86, Fascisti dissidenti, Archivio Centrale dello Stato, for telegrams from the prefects of all of these provinces describing these events.

[12] This generalization is borne out by the reports of the prefects and by a growing number of local and regional studies, e.g., Luigi Abrizzani, "L'avvento del fascismo nel Bolognese," *Movimento operaio e socialista,* X (Apr.–June, July–Dec. 1964), 83–102, 253–76; Antonio Bernieri, "Il fascismo a Carrara tra il 1919 e il 1931," *ibid.* (Jan.–Mar., Apr.–June 1964), 39–55, 105–19; Arrigo Cervetto, "Dopoguerra rosso e avvento del fascismo a Savona," *Rivista storica del socialismo,* I (Oct.–Dec. 1958), 511–62; Secondo Ramella, *L'azione sindacale nell'agro*

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1919, but even on the local level they were divided and poorly organized. The "occupation of the factories" in September 1920 had frightened the propertied classes without advancing the cause of the workers; the ambition of certain Socialists, especially in Emilia, to "build socialism in one province" had been equally unsuccessful; the internal quarrels within the Socialist party and the Communist secession from it at the Livorno Congress in January 1921 demoralized the workers at the very moment when the Fascists and their reactionary backers were mobilizing their strength. Yet it would be wrong to view *squadristi* violence and strike-breaking activities merely as a reaction against the Red Scare. Balbo, Farinacci, Ricci, and other leading ras used these tactics as a means of making themselves dictators in their own provinces.

The squadrism that brought Mussolini to power on October 30, 1922, did not disappear after the Black Shirts went home peacefully two days later. While Mussolini seemed content to be Prime Minister of a coalition government and to leave the existing order intact, Farinacci, Balbo, and even Michele Bianchi, the national party secretary, viewed the march on Rome as only the beginning of their conquest.[13] On the other hand, half of the sixty thousand party members in August 1922 resigned within a year, particularly those rank-and-file *squadristi* from the rural areas of the Po Valley, many of whom were the sons of landowners, including small proprietors who had recently bought land from large landowners who feared a Red take-over.[14] These people left the party because they believed the Bolshevik danger to be over; others did so because they disapproved of the directives of the central party hierarchy after the march on Rome. (These defections were, however, more than equaled by late joiners.) One of these directives ordered all *squadristi* to become members of the newly created Fascist militia (*Milizia Volontaria per la Sicurezza Nazionale*). Although Mussolini himself looked upon the militia as a way of bringing the local Black Shirts under the control of the government, some of its leaders wanted to transform it into a praetorian guard that would dictate both party and government policy.

Unlike the situation in October 1922, when Mussolini himself had de-

novarese dal 1918 al 1925: Socialisti e fascisti a confronto (Novara, 1962); Mario Vaini, *Le origini del fascismo a Mantova, 1914–1922* (Rome, 1961). In the South and in Sicily and Sardinia there were fewer Fascists than in the North, and their main rivals were often the Nationalists. (See Luigi Nieddu, *Origini del fascismo in Sardegna* [Cagliari, 1964].) The situation in Naples, however, tended to resemble that of the northern industrial cities. (See Raffaele Colapietra, *Napoli tra dopoguerra e fascismo* [Milan, 1962].)

[13] Massimo Rocca (Libero Tancredi), *Come il fascismo divenne una dittatura* (Milan, 1952), 127. Rocca was a high-ranking party member until his expulsion in 1924 because of his "revisionism."

[14] *Ibid.*, 161–62.

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cided that the *squadristi* should march on Rome, by the end of December 1924 it was they who forced him to "complete the revolution." Ever since the brutal murder of Matteotti in June, Mussolini had seemed uncertain about how to stay in power in the face of public protests and possible dismissal by the King. By mid-December he even proposed restoring the pre-1923 electoral law as a means of placating his opponents in Parliament. Such acts of "normalization" prompted several high-ranking leaders of the militia, followers of Farinacci,[15] and other *squadristi* extremists to intimidate Mussolini into making a *coup d'état*. Under pressure from this "cartel"[16] he set up a dictatorial regime in his speech to the Chamber of Deputies on January 3, 1925, and made Farinacci the national party secretary a month later. The *squadristi* precipitated a new outburst of violence that lasted until early 1926. But this "second wave" was their swan song, for the very dictatorship they had forced the *Duce* to establish gave him the power, through the prefects, to tame them and disperse most of their leaders.

The idea that squadrism was a kind of "feudal" resistance against the central government only partially explains its goals. Its proponents certainly wanted political control in their own provinces, but they also had national goals for the party. On December 28, 1924, Curzio Suckert Malaparte had stated these goals in their most extreme form in his "nine points" of integral Fascism in his newspaper *La conquista dello stato* (Florence). According to Suckert the march on Rome and the Fascist electoral victory in April 1924 had been only the first steps of the "Fascist provincial revolution toward the definitive conquest of the liberal state in order to arrive at the creation of the unitary national state." Farinacci, writing in *Il Popolo d'Italia* on October 20, 1925, when he was the national party secretary, said that the goals of squadrism were "the legalization of Fascist illegality . . . exceptional laws for the defense of our revolution . . . reforms for the great Fascist reconstruction." A year later he set forth the goal of an elitist party of 250–300,000 members with its Grand Council elected only by party members in each district.[17] In other words, the proponents of squadrism who survived until 1925 wanted to make the national party their instrument both for assuring their local power and for destroying the institutions of the liberal state. Many also remained anticlerical and republican, although they usually did not attack the Church and the monarchy directly.

The methods of many leading ras were as brutal as those of Al Capone,

[15] There is no evidence that Farinacci himself was involved in the movement led by the militia consuls, but a number of his followers knew of it and tried to exploit it. (See Adrian Lyttelton, "Fascism in Italy: The Second Wave," *Journal of Contemporary History*, I [No. 1, 1966], 95.)

[16] De Felice, *Mussolini il fascista*, 711.

[17] Cited in Tamaro, *Venti anni di storia*, II, 156.

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but these men were not ordinary gangsters. They wanted their gangs to take over the whole country, punish all those citizens whom they considered subversive, and indoctrinate the rest with a superpatriotic military spirit. Balbo's famous comparison between the activities of the *squadristi* and the Albigensian Crusade was not merely high-flown rhetoric. The main difference—and it was crucial—between the *squadristi* and the northern French crusaders was that the latter were aristocrats in the service of the King, whereas the former were plebeians trying to destroy the legitimate regime as well as root out subversives. Hence the *squadristi* were revolutionaries in a way that Capone and Simon de Montfort were not, despite the common element of brutality. Their revolution, however, was to be political; the old ruling groups were to be displaced by a new rough-and-ready elite, but the economic and social structures were not to be altered.

The *squadristi,* like all other Fascists, argued that they were making a "spiritual" rather than a materialist revolution, but since Mussolini stripped them of their remaining political power after 1925 they never had the chance to prove their case. By the early 1930's the only original ras still entrenched in his bailiwick was Farinacci, one of the least "spiritually" inclined of the lot. Ricci was the head of the Balilla from 1926 to 1937 and thus had the best opportunity to instill a new spirit in the children, but his rivalries with the leaders of other Fascist youth groups,[18] plus the unimaginative character of the Balilla's Saturday afternoon exercises, produced dubious results. Of the other leading ras, Augusto Turati (Brescia) served as party secretary from 1926 to 1931 and then faded from public life; Leandro Arpinati (Bologna) became a good bureaucrat as Undersecretary of the Interior until his disgrace and expulsion from the party in 1933. Mussolini had tried to "tame" Balbo early in 1923 by making him one of the top commanders of the militia, but the rivalry between the two men appeared even then when, in the meeting of the Grand Council of January 12, Balbo openly asked Mussolini if "the revolution has been made for you alone or for all of us."[19] He served as Air Minister in the late 1920's, but his spectacular transatlantic flights in 1930 and 1933 made him so popular that Mussolini got rid of him by sending him off to govern Libya.

Although the *squadristi* could not have realized their youthful ideal of regenerating Italy's "sick society" either with or without Mussolini, they did give Fascism its heroic, revolutionary, D'Annunzian stance. Mussolini himself openly assumed this posture in early August 1924 when he said that

[18] This was especially true of Carlo Scorza's *Fasci Giovanili* beginning in 1931. (See Ricci to Mussolini, May 30, 1931, Seg. Part. del Duce, 242/R, Riunioni del Direttorio del Partito Nazionale Fascista, sottofasc. I, in which Ricci complained that Scorza was trying to enroll fourteen–eighteen-year-old students in the *Fasci Giovanili,* thus robbing the Balilla of them.

[19] Rocca, *Come il fascismo divenne una dittatura,* 122.

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all true Fascists should be proud to reach their goal unadorned by titles and honorific trappings (*arrivare nudi alla mèta*) and that their motto should be: Live dangerously (*vivere pericolosamente*). The *squadristi* marching song, "Giovinezza, Giovinezza," which became the second national anthem along with the "Marcia Reale," vaunted bomb throwing and dagger wielding as well as youth as the springtime of beauty. Violence, youthful dreams, and blustering heroics were not unique to Fascism; they had a long tradition in Italy, and the *squadristi* openly borrowed much of their ritual, including the battle cry *me ne frego,* from Gabriele D'Annunzio's Fiume legions. The point is that many of them took these things quite seriously. Just as the goals of militarism are the glorification of the military way of life and its imposition on the state and the civilian population, so the goals of squadrism were the glorification of its way of life and its imposition on the state and the civilian population. The fact that the *squadristi* also thought of themselves as revolutionary activists gave them a *mystique* that the regime preserved—albeit as mere rhetoric—long after they themselves were either tamed or purged.

Whereas the ideal of the *squadristi* was a demagogic, gangster-like form of militarism, the goal of the militant Fascist trade-union leaders was a heretical version of revolutionary syndicalism.[20] Almost all these leaders had been involved in revolutionary trade-union activity in the immediate prewar years. Edmondo Rossoni (along with Michele Bianchi and Alceste De Ambris) had organized syndicalist strikes in the Po Valley in 1907–1913 and in the fall of 1914 had broken away from the anarchist-oriented *Unione Sindacale Italiana,* which remained pacifist, to form the *Unione Italiana di Lavoro* and to work for the interventionist cause. Mario Rachelli, Luigi Razza, and other Fascist syndicalist leaders had been associated with Filippo Corridoni, the Milanese revolutionary syndicalist and "poet of the masses," who by late 1914, along with Mussolini, was advocating war and revolution in order to replace the "decadent" liberal regime with a new society organized around cooperation among classes. Soon after Mussolini had founded *Il Popolo d'Italia,* a number of these syndicalists wrote periodically for it, but they had no common belief. Unlike Mussolini and Corridoni, Agostino Lanzillo, a journalist who was later to become the leading Fascist spokesman in Parliament, was for the war but not for a revolution. Even De Ambris, whose socialistic Charter of Carnaro in September 1920 rallied many of the most militant syndicalists to D'Annunzio's regime in Fiume, had written articles

[20] Some observers see this aspect of Fascism, especially in the beginning, rather as a Marxist heresy. (See Giovan Battista Chiesa, "Il 'Patto col diavolo' di Alfredo Rocco," *Rivista trimestrale,* IV [Mar.–June 1965], 178–200; and Augusto Del Noce, "Idee per l'interpretazione del fascismo," in Casucci, *Il fascismo,* 370–83.)

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in *Il Popolo d'Italia* earlier that year. Only by the beginning of 1922 did the various Fascist syndicalists adopt a fairly consistent set of goals.

Fascist syndicalism paid lip service to the Nationalist ideal of cooperation among classes while retaining its faith in an autonomous labor movement as the main force capable of completing the political revolution with a social revolution. Rossoni, the relatively moderate head of the Fascist trade unions, said in a speech in Naples in December 1922: "Between Italians and Italians there should be neither masters nor servants, but loyal collaborators for the common interest and for the overriding ends of the Fatherland."[21] He then added that "against the 'bosses' in the old sense of the word we shall fight ruthlessly." Three years later, when the Fascist unions were being given a legal monopoly in representing labor, Lanzillo told the Chamber of Deputies that these "unitary" unions were "a new reality alongside the family, the city, the commune, the State."[22] Lanzillo went on to say that the regime should not fear their revolutionary function in realizing the complete program of syndicalism, namely, their development into a structure parallel to the state and, ultimately, their assuming certain responsibilities of the state in the collective administration of society.

From a Marxist point of view revolutionary syndicalism had been a heresy ever since Pierre-Joseph Proudhon and Sorel; from any point of view Fascist syndicalism was a perversion of revolutionary syndicalism in two ways: it substituted national solidarity for the solidarity of the working class; and syndicalist goals were to be imposed from above rather than achieved by revolution from below.[23] The Palazzo Vidoni Pact in October 1925 and the labor laws of 1926 were victories for the employers, not the workers. By barring strikes and insisting on compulsory arbitration of labor disputes, these laws prevented the Fascist unions from engaging in effective collective bargaining, let alone changing the economic and social structures. In his aforementioned speech to the Chamber of Deputies, Lanzillo expressed his trepidations on both counts: "Excessive control will transform the unions into bureaucratic organs and their leaders into functionaries, thus frustrating the revolutionary spirit of the law. . . . It is absurd to wish to deny the possibility of economic strikes." In cases of substantive disputes between workers and employers, "it is force that must decide." The breakup of the National Confederation of Fascist Unions into six occupational confederations in Novem-

[21] Edmondo Rossoni, *Le idee della ricostruzione: Discorsi sul sindacalismo fascista* (Florence, 1923), 31.

[22] Meeting of Dec. 5, 1925, Atti del Parlamento italiano, Camera, Legislatura XXVII, Sessione 1924–25, Discussioni, V, 4849–51.

[23] The imposition of a new, antibourgeois, economic and social order from above was already suggested in September 1921 by Pietro Gorgoloni in *Il fascismo nella vita italiana* (Turin, 1923), 33. Rossoni, however, wanted the Fascist workers to achieve syndicalist goals through their own organizations.

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ber 1928 and the subsequent law setting up mixed corporations of workers and employers ended all hopes of making Fascist syndicalism a vital force in Italian life.

These hopes had already been vitiated by the actual means the Fascists used to gain power. From late 1920 until the march on Rome Rossoni, Rachelli, and other Fascist syndicalist leaders had to concentrate on the immediate task of administering the Fascist unions of small farmers that the leading ras in the North sponsored in order to compete with the older landowners' associations in destroying the unions of agricultural laborers. In Cremona, Farinacci forced some small farmers to join, but he opposed real working-class syndicalism.[24] In Ferrara, Balbo helped to found the first Fascist unions and persuaded Rossoni to come from Rome to help him; in January 1922 Rossoni established the first confederation of all Fascist unions at Bologna, where Rachelli was setting up the national headquarters of the farmers' unions.[25] In Bologna, Arpinati denied that the farmers' unions in his province represented a bourgeois reaction, but the leading Fascist history of the rural unions describes the two founders of the Bologna federation as sons of big landowners[26] and Rachelli as a man who, "after having vainly tried with De Ambris and others to reconstitute alongside the Fascist movement the ranks of revolutionary syndicalism inspired by the Charter of Carnaro," threw himself into his new job of organizing the farmers.[27] The Fascist syndicalists opposed *squadristi* violence against the Socialist and Catholic unions of agricultural workers,[28] but it was the destruction of these unions and the attacks against the urban labor movement that gave the Fascists their mass following among the middle and lower middle classes as well as tacit support from the police and the army.

After the march on Rome both the *squadristi* and the syndicalists tried to pursue their own goals in opposition to each other and often to Mussolini himself. From the beginning, however, the syndicalists were handicapped by the determination of the ras to keep them under their own control. Thus, long before the state tamed the Fascist unions in 1926, they had to fight an uphill battle against individual ras who, as federal secretaries of the party, wanted to have their own headquarters run all the local mass organizations, including the unions. The conflict with the ras and with the regime itself reached its peak in December 1924, when both the militant syndicalists and the extremist *squadristi* leaders were each trying to force Mussolini to fulfill

[24] Rocca, *Come il fascismo divenne una dittatura,* 144.
[25] Edoardo Malusardi, *Elementi di storia del sindacalismo fascista* (3d ed., Lanciano, 1938), 45. Despite its propagandistic flavor this account is accurate about specific facts.
[26] Giovanni Pesce, *La marcia dei rurali* (Rome, 1929), 115.
[27] *Ibid.,* 157.
[28] Rocca, *Come il fascismo divenne una dittatura,* 84.

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their differing versions of the "revolution." Though *squadristi* pressure was undoubtedly more crucial in making Mussolini set up a dictatorship in his speech of January 3, 1925, syndicalist pressure also had some effect in showing him how isolated he was from the militants in the Fascist movement.

Rossoni summed up the views of the leading syndicalists in a report to Mussolini at the end of 1924.[29] On the question of Mussolini's failure to see syndicalism as the motive force of the Fascist revolution and to fulfill this revolution, Rossoni mentioned an interview of his colleague Arnaldo Fioretti in the December 3, 1924, issue of *Nuovo Paese*. According to Fioretti, Fascism should carry out in the political field the goals of the Fascist union movement just as socialism had done for the Socialist Confederation of Labor. Fioretti's torment over the "arrested revolution" led him to consider making the unions autonomous from the party, even to the point of abandoning the label Fascist. Rachelli and Razza shared this view, but Rossoni, while reporting it, asserted that he wanted to remain loyal to Mussolini and the party.[30] Rossoni agreed with his colleagues, however, in their opposition to control of the provincial union federations by the ras.[31]

Despite his loyalty to Mussolini and the party Rossoni continued to champion the workers' interests against those of the employers. During the winter of 1925–1926 he urged the Fascist Grand Council to weaken the *Confindustria* (industrial employers' trade association) by setting up mixed corporations in which workers and technicians would participate in the operation of large firms. Antonio Benni, the *Confindustria* representative on the Grand Council, opposed this idea,[32] and his view concerning noninterference with the employers' activities prevailed throughout the Fascist period. In the fall of 1927 Rossoni was still fighting for working-class interests when he complained about high prices, despite the overvaluation of the lira, in a speech to the agricultural workers in the province of Milan. Mussolini rebuked him for this speech,[33] and on November 3, 1927, the *Corriere della Sera,* spokesman for the business community, criticized his general defense of the unions

[29] Rossoni, résumé of the meeting of the directorate of the *Confederazione delle Corporazioni Fascisti,* Dec. 30–31, 1924, Seg. Part. del Duce, W/R, sottofasc. 3.

[30] See also Rossoni's lead editorial in *La Stirpe,* III (Jan. 1925), where he too insisted that syndicalism was an integral part of the Fascist revolution and essential for the realization of that revolution.

[31] Rossoni's position at this time was confirmed to me by Riccardo Del Giudice in a personal interview on November 10, 1967. Del Giudice was active in the Fascist labor movement throughout its history and was the head of the national Confederation of Commercial Employees from 1933 to 1939. He remained on friendly terms with Rossoni until the latter's death in 1965 and is a professor of labor law at the University of Rome. According to him, until 1926 Rossoni believed in a rather nebulous syndicalist corporatism in which unions of workers and unions of employers working together would give Italy a new spirit. Rossoni wanted the labor movement to be autonomous from the party at the local level, with himself as the link between the two at the national level.

[32] Memo, Benni to Mussolini, Mar. 30, 1926, Seg. Part. del Duce, Gran Consiglio, 242/R, fasc. 20, sottofasc. 4 (1926), inserto C.

[33] Copy of a letter from Mussolini to Rossoni, Sept. 20, 1927, *ibid.*, W/R sottofasc. 3,

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and the workers' demands. In reply to this editorial Rossoni reiterated his own position in the November 5 issue of *Il Lavoro d'Italia*.

But Rossoni's influence soon ended: in addition to opposition from the industrialists and Mussolini's irritation at his nickname of "*Duce* of Fascist syndicalism," Rossoni faced the determination of top party leaders to end the class-conscious outlook of the Fascist unions. In November 1928 Giuseppe Bottai and Augusto Turati persuaded the Fascist Grand Council to break up Rossoni's confederation and to force him out of the union movement. Bottai and Turati also triumphed with their motion to reject a proposal by Fioretti to legalize shop stewards at the September 9, 1929, meeting of the Central Inter-Union Committee, which was presided over by Mussolini himself.[34] This decision not only shelved the question of the shop stewards for a decade; it also showed the effects of periodic purges of the more militant union leaders in the province beginning in the mid-1920's, for the majority of the current union leaders themselves voted for the Bottai-Turati motion.

After 1929, when the Fascist unions were gradually absorbed into Rocco's "corporate state," spokesmen for both the old *squadristi* position and the old syndicalist position occasionally spoke out against this trend. Toward the end of his term as national party secretary Turati told Mussolini that the growing bureaucracy of the Ministry of Corporations was stifling the activities of the unions and recommended restoring the authority of the provincial party secretaries in union matters.[35] In late 1933 Luigi Razza, head of the Confederation of Agricultural Workers, accused "antirevolutionary" elements (that is, Rocco) in high places of wanting to use the corporations as a means of preventing the unions from giving the masses a political consciousness, just as they had already succeeded in taking all political power away from the party itself.[36] In 1939 the Fascist unions benefited indirectly from Mussolini's renewed irritation toward the bourgeoisie (mainly because of its opposition to his aggressive foreign policy) by finally being given their shop stewards. But this decision was initiated by the state, despite Capoferri's claims that it was he who persuaded Mussolini to take it.[37] Fascist syndicalism had, in reality, lost its main thrust in the mid-1920's.

Beginning in 1925 Mussolini's regime instituted its own form of *Gleichschaltung*. As in Nazi Germany eight years later, this process involved the

[34] Alberto Aquarone, "La politica sindacale del fascismo," *Il Nuovo Osservatore*, Nos. 44–45 (Nov.–Dec. 1965), 881.

[35] Turati to Mussolini, Jan. 1, 1930, Seg. Part. del Duce, Gran Consiglio, 242/R, sottofasc. 8, inserto A.

[36] Luigi Razza, "La corporazione nello Stato fascista," in *Quaderni della rivista La Terra* (Rome, 1933), 51–52.

[37] Capoferri, *Venti anni col fascismo*, 151.

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elimination of the opposition political parties and independent labor unions and the "coordination" of the press, the schools, the courts, and other public agencies. Unlike its counterpart in Nazi Germany it also included the subordination to the state of the party itself and the organizations it sponsored: militia, Balilla, *Dopolavoro,* and so forth. Mussolini strengthened the faltering state administration he inherited, especially the Ministry of the Interior; in Germany friction between Nazi party organizations and the traditionally strong state administration weakened the latter.[38] Martin Bormann had far more discretionary powers than any Fascist party secretary; Robert Ley's Labor Front was able to challenge the *Reichsbank,* the Ministry of Economics, and the party hierarchy itself, whereas the Fascist unions and corporations could put no real pressure on anyone except the workers. Yet despite the Fascists' subordination of other agencies to the state, their attempt to create a totalitarian regime had less success than that of the Nazis. One reason was that Mussolini never completely eliminated the influence of the King and the Pope; another reason was that, unlike Hitler, he delegated the task of his *Gleichschaltung* to men whose outlook was quite different from his: Federzoni (Minister of the Interior, 1924–1926), Rocco (Minister of Justice, 1925–1932), and Arturo Bocchini (Chief of Police, 1925–1940).

Of these three men it is Rocco who interests us here, since he became known as "the official theorist of Fascism." Federzoni was a traditional authoritarian nationalist; Bocchini was an efficient, apolitical police chief, loyal only to the *Duce* and determined to eliminate all troublemakers, including high-ranking party members.[39] Rocco was an eminent professor of law and an original thinker. By 1914 he had made his reactionary elitist ideas prevail in the Italian Nationalist Association. He had two basic goals: to substitute an authoritarian state for the liberal state and to replace the unsupervised conflict of social and economic forces with a hierarchical class structure and with corporations for each sector of the economy, which would run their own affairs, but would have to submit to the authority of the state. Rocco incorporated these goals into a full-blown, logically consistent ideology.

Because Mussolini gave Rocco the power to make this ideology the constitutional basis of the Fascist regime, a number of observers argued that the Fascists, having no ideology of their own, took over the Nationalist program. This argument overlooks both the difference between the façade and the praxis of the regime and the divergence of Mussolini's goals from those of other Fascists. Mussolini wanted a personal dictatorship based on mass

[38] See Hans Mommsen, *Beamtentum im Dritten Reich* (Stuttgart, 1966).

[39] For example, in a letter dated December 2, 1932, Farinacci complained to Mussolini about his continual harassment by the OVRA, the political division of Bocchini's police force. (See Seg. Part. del Duce, 242/R, Farinacci, sottofasc. 6, inserto B.)

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support for his charismatic leadership ("Mussolini is always right") and reinforced by a strong police state; most other Fascists wanted something more, although this was all that materialized. Nevertheless, in 1926, having accepted Rocco's program of reform, Mussolini launched the famous slogan: "Everything in the state, nothing against the state, nothing outside the state." Mussolini was mainly concerned with the second goal—eliminating the enemies of his regime and neutralizing those forces, especially the monarchy and the Church, that he could not eliminate. Rocco's ideology, on the other hand, was midway between that of the Saint-Simonians and Comtians of a hundred years earlier and the truly totalitarian regimes of today. His ideal society was to be run by the *grands commis de l'État* and the progressive captains of industry, with Auguste Comte's primacy of the social organism as the new principle of legitimacy.[40] Despite Mussolini's slogan, Rocco's conception of the state was reactionary rather than totalitarian, authoritarian rather than demagogic.

In a pamphlet published in 1914 Rocco tried to distinguish his reactionary nationalism from the French variety by arguing that he did not want to revert to the days of absolutist monarchy in alliance with the Church.[41] He went on to say that the goals of Charles Maurras were defensive and domestic, as was suitable to a rich country with a declining population, whereas Italian nationalism was imperialistic, as was proper in a poor country with a burgeoning population. Yet despite all his assertions about institutions having to fit Italy's historical peculiarities Rocco wanted to impose a centralized system of state absolutism closer to that of Louis XIV and Colbert, which was certainly not what Maurras wanted. The doctrinaire hostility of the Vatican to the Italian state made many nationalists anticlerical; Rocco was also suspicious of the Church for the same reason that he opposed Freemasonry, which he considered as internationalist and pacifist,[42] as well as being an occult intermediary organization between the state and its subjects.

Rocco's so-called corporatism was neomercantilist. Unlike Maurras, he disapproved of autonomous intermediary organizations like corporations, communes, estates, and the like. For Rocco, "nothing outside the state" in the economic sphere meant neither state socialism nor state capitalism; it meant the reorganization of big business in a way that would avoid "useless internal competition in order to face, under conditions of maximum economic efficiency, the battles of international competition."[43] Rocco wanted to elim-

[40] Paolo Ungari, *Alfredo Rocco e l'ideologia giuridica del fascismo* (Brescia, 1963), 32.
[41] Alfredo Rocco, "Che cosa è il nazionalismo e che cosa vogliono i nazionalisti," in *Scritti e discorsi politici* (3 vols., Milan, 1938), I, 88.
[42] *Id.*, "Nazionalismo e fascismo," speech of Feb. 25, 1923, *ibid.*, II, 729.
[43] *Id.*, "Il momento economico e sociale" (1919), *ibid.*, 586.

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inate all working-class autonomy, privileges, and influence in productive enterprises, especially those gains made in 1919 and 1920. Unlike Saint-Simon, Rocco did not believe in the possibility of harmony among classes. Defending strict state control over his "mixed unions" of employers and workers against Lanzillo's criticism, Rocco told the Chamber of Deputies: "The State, least of all the Fascist State, cannot permit the constitution of States within the State. The organization [in the 1926 laws] of the unions must be a means of disciplining the unions, not a means of creating strong, uncontrolled organisms capable of undermining the State."[44] In one of his last public speeches he said that, in contrast to the medieval corporation, the Fascist corporation achieves a high level of discipline in the productive process not only in the interests of the producers

> but above all in the general interest, under the effective tutelage of the State . . . as an organ of the State. . . . Utilizing its technical competence and the stimulus of the individual interests of the producers it operates above all to make the production and hence the wealth of the Nation more perfect, more profitable, more considerable.[45]

In other words, "What's good for General Motors is good for the country."

Rocco never saw his neomercantilist dream realized, partly because of the resistance of big business to state interference, partly because the depression halted the kind of juridical reforms he wanted, and partly because his economic and social goals interested neither Mussolini nor most other Fascists. The so-called corporate state that was finally achieved on paper in 1934 changed nothing; it merely continued the practices established in the 1926 laws disciplining labor without controlling management. It was his political goals, most of which *were* achieved[46] in laws devised by him between 1925 and 1932, that made Rocco the "official theorist" of Mussolini's variety of Fascism.

These laws reinforced the *Duce*'s dictatorial powers by allowing him to legislate by decree, by outlawing all other political parties, by eliminating the possibility of subversion, by "demoting" the Fascist party from an autonomous "private" organization to a propaganda agency of the government, and by making the Fascist Grand Council Mussolini's "general staff." Even the government itself was reduced to the status of a merely consultative and executive organ without joint responsibility for decisions taken by its head. Increasingly, especially after the law of December 9, 1928, defining its or-

[44] *Id.*, speech of Dec. 10, 1925, *ibid.*, III, 991.

[45] *Id.*, "Costituzione e funzioni delle Corporazioni," speech to the Chamber of Deputies, Jan. 16, 1934, *ibid.*, 1012.

[46] The Lateran Treaty of 1929 was the only major setback to Rocco's political program, and he did not hide his disapproval of the "abdication" to the Vatican of parts of the state's authority, particularly concerning civil marriage.

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ganization and powers, the Grand Council became the highest deliberative organ of the regime. Not only was its approval necessary for all further constitutional changes; it also prepared the lists of deputies to be elected and union leaders to be appointed and was even empowered to rule on the succession to the throne and to choose Mussolini's eventual successor.

Mussolini and most party leaders lauded Rocco's concentration of all political power in the hands of the *Duce* and the Grand Council, but Rocco had envisioned a different kind of elite from the one that actually ran the Fascist regime from the early 1930's until its final demise. When Rocco left the Ministry of Justice in 1932 most of Mussolini's ablest collaborators were dismissed as well, including Giuseppe Volpi as Minister of Finance, Dino Grandi as Minister of Foreign Affairs, Giuseppe Bottai as Minister of Corporations, and Balbino Giuliano as Minister of Education. Thereafter the leading government ministers were mainly party hacks, although Bottai returned to office as Minister of Education in 1936. Rocco had wanted an elite of highly competent civil servants, progressive industrial tycoons, and "Solons" like himself. Instead the leading figures of the regime after 1932 were "tamed" former *squadristi* like Achille Starace, Renato Ricci, Ettore Muty, and Carlo Scorza. These and other untutored *gerarchi* flanking the *Duce* on all public occasions became the image of the Fascist elite for most Italians.

The inability of Fascism to produce a capable new ruling elite prevented it from realizing any of its diverse goals. Even Mussolini himself, despite his undeniable qualities of leadership and attraction for the masses, did not have the intelligence or character of a truly great and creative statesman. One of his most astute colleagues compared him to "an electric power station that illuminated one small lamp bulb . . . an energy that dispersed itself and evaporated for want of collecting centers, of links that might articulate it."[47] In Weberian language Fascism was an authoritarian dictatorship in which the charisma of the leader did not become bureaucratized. This failure was due in part to the compromises Mussolini had to make with the older elites—monarchy, Vatican, army, state administration, big business—all of which endured alongside of the new hierarchical party organizations. It was also the result of his inability to reconcile his mania for making all political decisions himself with his growing indulgence toward the proliferation of rival administrative structures. Shorn of real political initiative, the party *gerarchi* were not forced to work out a unified ideology that they could hope to impose on the older elites and the masses.

[47] Giuseppe Bottai, *Vent'anni e un giorno* (Milan, 1949), 31.

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Before concluding with some of the ideological efforts to revive the "continuing revolution" within the framework of the bureaucratized regime during the 1930's, we must dispose of Gentile's conception of the "ethical state." This conception served more as the wishful thinking than as the working ideology of Fascism, but it must be mentioned briefly because Mussolini himself followed it almost literally in his famous article on "Fascism" in the 1932 edition of the *Enciclopedia italiana*. The controversy over whether or not Gentile ghost-wrote this article is unimportant here. Certainly the first part, "Dottrina-idee fondamentali," is borrowed completely from Gentile's 1925 essay *Che cosa è il fascismo*. The second part, "Dottrina politica e sociale," diverges from Gentile in its emphasis on militarism, in its conception of the state as an entity independent of its citizens, and in its identification of the Fascist state with the Roman tradition. Gentile, of course, was the editor in chief of the encyclopedia, but it is unlikely that he edited Mussolini.

The first part of Mussolini's article assumes the basic premise of Gentile's "actual idealism": the identity of thought and action. From this premise Mussolini postulates the idea of an authentic community which Fascism is trying to make actual by fusing the individual and the nation. Only in such a community can the individual realize his true potentialities, which are spiritual rather than material, social rather than individual. The means for making this ideal a reality is the totalitarian state.

> Fascism reaffirms the state as the true reality of the individual. . . . For the Fascist everything is in the state, and nothing human or spiritual exists, much less has value, outside the state. In this sense Fascism is totalitarian, and the Fascist state, which is the synthesis and unity of all values, interprets, develops, and brings out the full potential of the total life of the people.

Thus the Fascist state is an *ethical* state because it alone makes possible the realization of true human values. "The nation as state is an ethical reality that exists and lives as long as it develops."

Mussolini liked Gentile's idea that the Fascist regime was the means of creating a new type of Italian and a new civilization. According to Gentile, the spiritual essence of Fascism, its "religious, totalitarian view of life," must inform the consciousness of all citizens and permeate every sphere of daily life.[48] The idealist philosopher maintained that "politics and history are made not only by promulgating new laws, creating new institutions, or winning battles, but also (and properly so) by developing new states of mind, and ideas, in forming new men and a new spirit."[49] Fascism for him was neither a philosophy nor a dogma; it was, rather, a "continuing revolution" of the

[48] Giovanni Gentile, *Che cosa è il fascismo: Discorsi e polemiche* (Florence, 1925), 38.
[49] *Id.*, "La legge del Gran Consiglio," *Educazione fascista*, VI (Sept. 1928), 514.

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immanent spirit of the nation. It was bound by no irrevocable policies: "The true resolutions of the Duce are those that are both formulated and put into actual effect."[50]

This kind of language obviously flattered Mussolini, but he had no intention of making Gentile his Plato. First of all, he could not accept Gentile's idea that the state exists in so far as the citizens cause it to exist; for Mussolini and for all other leading Fascists, the state was autonomous. Furthermore, the "continuing revolution" was obviously over by the time of the Lateran Treaty. By then Gentile, who had been Mussolini's first Minister of Education and had headed an important constitutional commission, was relegated to purely academic posts and had no further influence on government policy. The hostility of the Vatican[51] also made it necessary to downgrade him in Fascist circles. He was, in addition, attacked by many party officials for harboring anti-Fascist professors on the editorial board of his encyclopedia. Thus, by the time Mussolini's article appeared in 1932 the man and the philosophy that had inspired it were already out of fashion.

In 1932 the "Fascist Revolution" was merely history and was enshrined in a public exhibition (*Mostra della Rivoluzione Fascista*) on the Via Nazionale in Rome. This exhibition depicted the events of 1919–1922, stressing the activities leading up to the march on Rome and thus making it perfectly clear that the revolution had been purely political.[52] The *squadristi* heroes of this revolution were canonized as the Old Guard (*Vecchia Guardia*), again emphasizing their place in the past rather than in the present. (Alessandro Blasetti's film *Vecchia Guardia,* released in 1934, was the best artistic effort to glorify squadrism as a historical phenomenon.) Anyone looking for evidence of Fascism as a "continuing revolution" did not find it in the 1932 exhibition or in any other feature of Italian public life. Squadrism and syndicalism were dead, the ethical state was an empty phrase, and corporatism was a façade for favoring the interests of the employers and the state over those of the workers.

But neither Mussolini, nor Starace (party secretary from 1931 to 1939), nor the old and new ideologues were content with the bureaucratized "cor-

[50] *Id.*, "Origini e dottrine del fascismo" (1927), cited in Casucci, *Il fascismo,* 37.

[51] In March 1930 Gentile had publicly attacked the Pope as a doctrinaire Thomist and cryptomaterialist. (See Police report, Mar. 15, 1930, Seg. Part. del Duce, 7R, Senatore Professore Giovanni Gentile, sottofasc. 1.)

[52] The main exhibits were on the ground floor; the first five rooms dealt with events from the outbreak of the First World War to the founding of the *Fasci Italiani di Combattimento* on March 23, 1919; the next ten rooms depicted the events from that date to the march on Rome, the last, and largest, hall being the "Shrine of the Martyrs" (*squadristi* killed during the "Revolution"). Only in five small rooms upstairs were there exhibits suggesting the accomplishments of the regime, and even here the emphasis was on things already done rather than things yet to be done. (See the 258-page illustrated catalogue of the *Mostra della Rivoluzione Fascista* [Rome, 1933].)

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porate state." Mussolini soon turned to imperialism. Starace wanted his version of the party's "Roman" style of behavior to become the national norm. The older ideologues, especially those who wrote for *Critica Fascista,* continued to call for the creation of an elite competent to deal with the problems of an industrial society. Bottai, the editor of *Critica Fascista,* Minister of Education from 1936 until early 1943, and editor of the literary review *Primato* (1940–1943), also wanted to give Fascism a new cultural base through his *Carta della Scuola* (1939), which tried to democratize the public schools and to bring life and work experience into the educational process, and through his efforts to patronize Italy's most talented artists and writers, regardless of their political views.[53] The most militant of the younger ideologues were divided into two opposite extremes: the neomystics (exponents of *mistica fascista*) and the "integral corporatists."

Despairing of all efforts to give Fascism a philosophical base, the neomystics rejected rationality in all its forms in favor of faith in the genius of Mussolini. Nicolò Giani, the director of the *Istituto di Mistica Fascista,* said: "We are mystics because we are mad . . . factious . . . and even absurd. Yes, absurd . . . [for] History has always been and will always be an absurdity —that is, mystical—the absurdity of the spirit and the will which bends and conquers the merely material."[54] Gianni Guizzardi said that "man is happy to abdicate his reason in order to believe in Him who leads . . . ," and he preached a "return to that most beautiful Myth, born in the bloodbath of the world war, which gave the first blow to modernity."[55] By "modernity" Guizzardi meant the rational, modernizing leadership of the liberal regime. In its place the neomystics substituted unquestioning faith in whatever Mussolini said or ordered them to do. *Credere, obbedire, combattere* ("believe, obey, fight") was a perfect slogan for their irrationalism, which was a vulgarized version of the "destruction of reason" proposed by certain philosophers and poets at the turn of the century; this slogan also had echoes of the *menefreghismo* of the original *squadristi.*

At the opposite extreme from these reactionary mystics a small number

[53] Bottai had come to Fascism via futurism, and he maintained his contacts with Italy's *avant-garde* writers and artists throughout the duration of the regime. In the late 1930's he gave academic posts to the poets Giuseppe Ungaretti, Salvatore Quasimodo, and Alfonso Gatto, the novelist Vasco Pratolini, and the painters Ottone Rosai and Giorgio Morandi. Gatto, who was a crypto-Communist then and has been an outspoken Communist since the days of the resistance, told me (personal interview, Nov. 28, 1967, in Rome) that Bottai did not patronize these artists and writers as a means of corrupting them into supporting the regime and that he remained personally loyal even to those who were anti-Fascist. This view was also expressed to me by Piero Bargellini, a conservative Catholic literary critic, writer of textbooks for the Fascist elementary schools, and mayor of Florence in 1966–1967, in a personal interview on July 9, 1966, in Florence.

[54] Nicolò Giani, "Perchè siamo dei mistici," *Gerarchia,* XIX (Feb. 1940), 113.

[55] Gianni Guizzardi, "Dalla regione alla fede," *ibid.,* 198.

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of radical students and older syndicalists were attracted to the seemingly ultramodern "integral corporatism" of the political philosopher Ugo Spirito.[56] In early May 1932, at a congress for corporative studies at Ferrara, Spirito sparked a lively debate over his unorthodox ideas. His basic premise was that Fascist corporatism had dealt "a mortal blow to the liberal conception of property" and that the result could only be the gradual fusion of capital and labor in all large enterprises. In this way the ownership of these enterprises would pass from the stockholders to the "producers," who would own and operate them according to their technical competence, thus eliminating the class struggle. Spirito himself later called this idea "communist,"[57] although it was actually closer to the tradition of Proudhon, the anarchosyndicalists, and the abortive factory committees set up in Russia immediately after the 1917 revolution, than to the state socialism of the Soviet Union in the 1930's. In any case Spirito believed that his conception of the *corporazione proprietaria* (a corporation owned in common by all who worked in it) expressed "the whole political, moral, and religious meaning of the Fascist Revolution." It is needless to say that the party *gerarchi* at the Ferrara Congress rejected Spirito's "Bolshevik" position, but a few of those present, including Mario Rachelli, then secretary of the Confederation of Commercial Workers, accepted it. During the late 1930's a self-styled "new Left" among the university students tried to use Spirito's ideas as a means of reviving the 1919 Fascist program's promise of radical social change.

The government permitted the expression of all sorts of heterodox ideas as long as these ideas did not challenge the regime itself, but it ignored them completely in formulating its own policies, especially imperialism. Although the conquest of Ethiopia rallied the Italian people behind the regime as no other Fascist policy did, it was not a uniquely Fascist goal. Mussolini's war with Ethiopia was popular not because it transformed Italy into a new Roman Empire but because it united the Italians against the opposition of the League of Nations and especially against the English. The *Duce*'s words and manner expressed the strong emotional need of millions of Italians to overcompensate for the patronizing and often contemptuous attitude of Anglo-Saxons, Frenchmen, and other Europeans toward them. The victory over Ethiopia seemed to fulfill this need until Mussolini spoiled the illusion that Italy could create its own "place in the sun" by making the country a junior partner of Germany.

[56] See Ruggero Zangrandi, *Il lungo viaggio attraverso il fascismo* (Milan, 1964), 442–45.
[57] Ugo Spirito, *Critica della democrazia* (Florence, 1963), 33. For a full statement of Spirito's position in 1932, see his "Individuo e Stato nella concezione corporativa," *Nuovi studi di diritto, economia e politica,* V (No. 2, 1932); for the evolution of Spirito's thought, see Antimo Negri, *Dal corporativismo all' umanesimo scientifico: Itinerario teoretico di Ugo Spirito* (Manduria, 1964).

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The disastrous end of their regime—defeated and overthrown in a war it could easily have avoided—has overshadowed the domestic goals of the Fascists during their rise to power. From 1919 until 1925 the majority of the Black Shirts were alienated young men, a self-styled "lost generation," determined to overthrow the liberal establishment by violent means and restore a "sick society" to health. Mussolini had already gone through this phase before 1914; he simply used the revolutionary activism of the *squadristi* to make himself a dictator. He also exploited the demagogic nationalism made popular by D'Annunzio (the archexample of the danger of the poet in politics) and the revolutionary syndicalist dreams of some of his older associates. Indeed his own revolutionary background made him think of himself as a Left-wing extremist up until late 1920. When he finally saw that such a role would get him nowhere, he abandoned it for that of a strike-breaking, anti-Bolshevik patriot. Yet once in office he could hardly be called a lackey of the existing "power structure."

To see Fascism primarily as an instrument of reactionary landowners and big businessmen is a misinterpretation of its basic significance. Balbo and Farinacci, Rossoni and Lanzillo, and even Mussolini himself hated the existing political, social, and cultural orders, whose leaders looked down on them as low-class rabble-rousers and who were only interested in using them in order to quell those revolutionaries who wanted to destroy the economic order as well. During the early 1920's the Fascists used the reactionary landowners and big businessmen as much as they were used by them in suppressing the Red menace. Their goal was not *counter*revolution but *another* revolution. The liberal regime was their ultimate enemy; in their struggle to subvert and overthrow it, they made the reactionaries their main allies and the Reds their main rivals. Their revolution destroyed the existing political system. The Fascists did not alter the class structure, but they made it possible for many young men of humble origin to rise to positions of power hitherto closed to them, and they gave many urban workers the illusions, albeit short-lived, of solidarity and of integration into the national society. They had nothing to offer culturally except empty rhetoric and a strained austerity in their style of public behavior. But then neither cultural poverty nor ideological disunity prevents people from being revolutionaries.

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The Cities and the Election of 1928: Partisan Realignment?

JEROME M. CLUBB and HOWARD W. ALLEN

IN recent years students of American political life have assigned particular significance to the presidential election of 1928. Various scholars have examined the voting patterns of the 1920's and have observed that major changes in these patterns occurred in that year. These changes have been interpreted as evidence that the election of 1928 was one of the rare "realigning" or "critical" elections in American political history that marked a lasting reorientation of the political behavior and partisan loyalties of major segments of the electorate. The voting shifts of 1928 have been interpreted, more specifically, as marking the beginning and in some ways providing the basis for the Democratic electoral majority that emerged in the 1930's and that apparently still remains substantially intact.

Samuel Lubell was probably the first to call attention to an "Al Smith Revolution" which antedated the "Roosevelt Revolution" of the 1930's.[1] Lubell noted a sharp increase both in the Democratic presidential vote and in the total vote in 1928 particularly in urban areas characterized by populations with a large immigrant stock. According to Lubell, this change was primarily attributable to the personal characteristics of the Democratic candidates and to peculiarities of the American social scene. As an urban Catholic of immigrant background, Alfred E. Smith was particularly appealing to the resentful and largely disaffected "new" immigrant population of urban America. Prior to 1928 many of these Americans had voted Republican, and more had refrained from participating in the electoral process. But in that year, primarily because of Smith, they went to the polls in unprecedented numbers, voted heavily Democratic, and continued to do so thereafter. Thus

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[1] Samuel Lubell, *The Future of American Politics* (2d rev. ed., New York, 1956), 35–43. Lubell's findings were anticipated in some respects by Samuel J. Eldersveld in "The Influence of Metropolitan Party Pluralities in Presidential Elections since 1920: A Study of Twelve Key Cities," *American Political Science Review,* LXIII (Dec. 1949), 1189–1206.

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one of the crucial elements of the "New Deal Coalition" actually moved into the Democratic camp in 1928.

More recently, various other scholars have submitted evidence that tends to confirm Lubell's findings. Irving Bernstein and Carl Degler have systematically compared the presidential vote in the major cities during the 1920's and have drawn conclusions essentially similar to those of Lubell.[2] Other investigators have also identified the election of 1928 as a "critical election" but with some reservations. On the basis of investigation of the presidential vote in New England, V. O. Key identified the election of 1928 as a "critical election" and concluded that in New England "the Roosevelt revolution of 1932 was in large measure an Al Smith revolution of 1928." Key added, however, that this characterization was "less applicable to the remainder of the country" and suggested that "the great reshuffling of voters that occurred in 1928 was perhaps the final and decisive stage in a process that had been under way for some time."[3] In their study of Illinois voting patterns Duncan MacRae, Jr., and James A. Meldrum also observed significant shifts in 1928, but they demonstrated that for Illinois at least it is more appropriate to speak of a "critical period" rather than a single "critical" or "realigning" election.[4] Although David Burner accepted the view that the election of 1928 had a significant and lasting impact upon the electoral behavior of some groups of immigrant stock, he pointed to the "deviant" nature of that election, as well as the elections of 1920 and 1924, and he raised the possibility that groups of immigrant stock had been deflected from their "normal course" in the presidential voting of the 1920's.[5] And even stronger reservations have been expressed. John L. Shover questioned this interpretation on several general grounds and on the basis of a detailed analysis of voting patterns in California has concluded that no major partisan realignment took place there in 1928.[6] But these reservations aside, the view that major and lasting political realignment occurred in 1928 in large measure because of the peculiar attributes of the Democratic presidential candidate has apparently been widely accepted.

The significance of this interpretation of the political history of the late

[2] Irving Bernstein, *The Lean Years: A History of the American Worker, 1920–1933* (Boston, 1960), 75–82; and Carl N. Degler, "American Political Parties and the Rise of the City: An Interpretation," *Journal of American History,* LI (June 1964), 41–59.

[3] V. O. Key, Jr., "A Theory of Critical Elections," *Journal of Politics,* XVII (Feb. 1955), 3–18.

[4] Duncan MacRae, Jr., and James A. Meldrum, "Critical Elections in Illinois: 1888–1958," *American Political Science Review,* LIV (Sept. 1960), 669–83.

[5] David Burner, *The Politics of Provincialism: The Democratic Party in Transition, 1918–1932* (New York, 1968), 217–43.

[6] John L. Shover, "Was 1928 a Critical Election in California?" *Pacific Northwest Quarterly,* LVIII (Oct. 1967), 196–204.

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1920's and the 1930's is obvious. If it can be demonstrated that major and lasting partisan realignment occurred in 1928 then it may be necessary to modify assessments of the political impact of the Great Depression and the policies of Franklin Roosevelt and the New Deal. This interpretation similarly lends weight to the view that ethnic, religious, and other essentially social factors have constituted major determinants of American political behavior and have perhaps been more important than factors of a more purely economic nature. Studies of contemporary political behavior and of historical voting patterns have suggested, moreover, that for much of the electorate partisan loyalties are deeply seated, intensely held, and remarkably stable over long periods of time.[7] Large-scale changes in party affiliation and habits of electoral participation have been rare and have been identified with times of major national crisis. If realignment came in 1928 as a result of developments less dramatic than national crisis, it may be necessary to modify views of the stability and intensity with which partisan loyalties have been held.

It is possible, however, to entertain doubts as to the adequacy of this general interpretation of the political events of the late 1920's and the 1930's. In the first place, the fact that Democratic successes in certain areas in the presidential election of 1932 were preceded by shifts toward the Democratic party in 1928 in those same areas does not demonstrate that these developments were related. It is possible that Smith's personal appeal brought about an upsurge in the Democratic vote in some areas without changing underlying partisan loyalties or patterns of voting behavior on the part of the groups involved. From this point of view, had it not been for the introduction of a new range of political stimuli by economic depression, the presidential vote would have returned in 1932 to the patterns of the years prior to 1928. Smith's successes may have been no more than a short-term fluctuation in the presidential vote without lasting importance in terms of underlying partisan divisions, and the election of 1928 could be more appropriately described as a "deviating" election rather than a "critical" or "realigning" election.[8]

Various considerations make this alternate interpretation of the election of 1928 at least logically plausible. The immigrant stock groups, which apparently provided the basis for Smith's gains in northern cities, were presumably among the lower income groups hit hardest by the Great Depression. Thus the fact that the Democratic candidate in 1932 was success-

[7] See, e.g., Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *Elections and the Political Order* (New York, 1966), esp. Chaps. II–IV; and Charles Sellers, "The Equilibrium Cycle in Two-Party Politics," *Public Opinion Quarterly*, XXIX (Spring 1965), 16–38.

[8] These possibilities are also suggested in Campbell, Converse, Miller, and Stokes, *Elections and the Political Order*, 75.

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ful in some of the same areas that shifted toward the Democrats in 1928 is compatible with the view that these developments were the result of different and unrelated factors. Large but temporary shifts in voting patterns have occurred, moreover, in American political history. Indeed, as has often been observed, in 1928 Smith split the Solid South. The Democratic vote in that region fell sharply, and large numbers of Democratic voters moved into the Republican column. Few have concluded, however, that the election of 1928 marked a lasting conversion of any significant number of southerners from Democratic to Republican affiliation. Better-known examples of "deviating" elections, perhaps, are those of the 1950's. The overwhelming victories of Dwight D. Eisenhower in 1952 and 1956 were hailed by some as an indication that a new Republican electoral majority had appeared. The events of the 1960's and the continued success of Democrats in their efforts to win election to lesser offices during the 1950's demonstrated that the Eisenhower victories were a tribute to personal popularity unaccompanied by fundamental realignment.

The electoral history of the Eisenhower years suggests a related limitation of the interpretation of the election of 1928 as a "critical" or "realigning" election. With the noteworthy exception of the work of MacRae and Meldrum, assessments of the voting patterns of the 1920's and 1930's, which provide the basis for this general interpretation, have considered only the presidential vote. The vote in 1928 has been compared with the vote in preceding and subsequent presidential elections, and in this way the electoral shifts and changes in voting patterns of 1928 have been identified. Consideration of the nature of the preceding and subsequent elections suggests that this approach may well have its shortcomings. The presidential election of 1924 was marked by the presence of a strong third-party candidate who certainly attracted many normally Democratic voters and who probably cut into Republican strength as well. Most investigators have found it necessary, therefore, to compare the election of 1928 with the election of 1920. It might be argued, however, that the presidential election of 1920 was unusual in character, for that election was a Republican landslide, and the Republican candidate received a larger percentage of the popular vote than any candidate prior to the election of 1936. In this respect the election of 1920 was outside the more normal pattern of American electoral politics. Comparisons with subsequent elections also present difficulties. Both the elections of 1932 and 1936 were conducted under conditions of severe economic stress and might also be considered outside the normal pattern of American elections. In short, the identification of electoral shifts peculiar to the election of 1928 has been based in some measure upon

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comparisons with elections that were unusual and marked, perhaps, by large but temporary shifts in voting patterns.[9]

The adequacy of the presidential vote as an indication of underlying political attitudes and deep-seated partisan divisions within the electorate can also be questioned on more general grounds. Presidential elections are often "high stimulus" elections that are accompanied by great political sound and fury. The issues of the day and the characteristics of the candidates are brought sharply to the attention of the electorate, and, in the twentieth century at least, voter turnout has usually been higher in presidential elections than in off year elections. It appears that presidential elections are particularly susceptible to the influence of short-term political forces that may shift the vote toward one or the other of the parties or toward particular candidates without bringing about a lasting change in basic electoral alignments. The circumstances of these elections often prompt some voters to abandon temporarily long-standing partisan loyalties and cross party lines. Conditions of high political stimulation usually bring large numbers of voters to the polls who normally do not vote in other elections, whose level of political interest is low, and whose political preferences are relatively unstable. These considerations work to complicate the voting patterns of presidential elections and lessen their utility as indications of fundamental and lasting divisions and alignments within the electorate.[10]

The voting patterns of subsequent years, therefore, appear to constitute a crucial test of whether major partisan realignment occurred in a particular election. In the case of the election of 1928 this test is, of course, complicated by the presence of a new range of political stimuli presented by the economic adversity of the early 1930's. It also seems, moreover, that the voting patterns of elections to lesser offices, particularly during nonpresidential years, provide a better indication of basic patterns of electoral behavior than do presidential elections. These elections are apparently less subject to short-term and essentially evanescent forces and more clearly reflect underlying partisan divisions and patterns of political participation within the electorate. In both respects, the elections of 1930 should provide a particularly critical indication of whether lasting realignment occurred in 1928.

Studies of the presidential vote during the 1920's and 1930's have shown that the Democratic vote and the total vote increased sharply in particular areas between 1920 and 1928. These increases have been interpreted as indi-

[9] This point is also made by Shover in "Was 1928 a Critical Election in California?" 196–204.

[10] The discussion in this and preceding paragraphs draws heavily upon Angus Campbell, Philip E. Converse, Warren E. Miller, and Donald E. Stokes, *The American Voter* (New York, 1960), *passim;* and *id., Elections and the Political Order,* esp. Chaps. II–IV. These authors, of course, bear no responsibility for our interpretation and use of their findings.

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cating that in these areas large numbers of voters came to the polls for the first time and voted disproportionately for the Democratic candidate. That these shifts were particularly noteworthy in urban areas with large immigrant stock populations makes it plausible to conclude that they were due to the peculiar appeal of Smith for dissatisfied Americans of immigrant stock. Even if this interpretation of the presidential returns is accepted, however, it does not follow that lasting changes in partisan affiliation and habits of political participation occurred in 1928. As has been seen, a sharp change in voting patterns associated with a single election does not in itself demonstrate that a permanent realignment has taken place.

These difficulties can be illustrated by examination of the vote in a limited number of areas during the 1920's and the earlier 1930's. Table I presents the Democratic percentage of the total vote for President in elections from 1920 through 1936 in major metropolitan areas that were characterized by many individuals of immigrant stock and that have been identified as marked by lasting shifts toward the Democratic party in 1928.[11] Inspection of the table clearly suggests that in these areas major shifts toward the Democratic party did take place in that year. In all of the areas considered Smith received a larger proportion of the total vote than did the Democratic candidates in 1920 and 1924. Although in most cases the Democratic vote rose to higher levels in 1932 and 1936, these gains were preceded by shifts toward the Democrats in 1928. In terms of the presidential vote alone in these areas, then, it is plausible to speak of an "Al Smith Revolution" preceding the "Roosevelt Revolution" of the 1930's.

When one examines the Democratic vote for candidates for lesser offices, however, he sees the shifts of 1928 in a different light. As Table II shows, shifts in the vote for Democratic candidates for lesser offices in the same areas during the same years followed a somewhat different pattern than did shifts in the presidential vote. In most of these areas, the vote for Democratic can-

[11] The cities listed in Table I and the following tables are identified by Degler as marked by major shifts toward the Democratic party in 1928. The cities are those of over 250,000 population in 1930 in which, with the exception of Los Angeles, Oakland, and Seattle, immigrant stock population (defined as foreign-born white and native white of foreign or mixed parentage) represented over 50 per cent of the total population. Although these groups accounted for less than 50 per cent of the population of Los Angeles, Oakland, and Seattle, these cities are included because the population of native stock (defined as native-born white of native parentage) also amounted to less than 50 per cent of the total population of each. The balance was made up by colored population. (See Degler, "American Political Parties," 53–56; and Bureau of Commerce, *Statistical Abstract of the United States, 1931* [Washington, D.C., 1931], 22–27.) As in the case of Degler's article, because election returns for the cities themselves are unavailable, the votes given are for the counties in which the cities were located. For purposes of clarity the city rather than the county names are used. The election returns used in this and the following tables were provided by the Inter-university Consortium for Political Research, which bears no responsibility for the analyses and interpretations presented here.

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Table I
Democratic Percentage of the Total Vote for President
in Selected Metropolitan Areas, 1920–1936

	1920	1924	1928	1932	1936
Boston	36.3	35.5	66.8	67.1	63.9
Providence	35.3	38.6	52.9	57.4	55.1
New York					
Bronx	24.4	33.6	67.7	70.4	67.2
Kings	25.9	31.9	59.5	66.9	63.6
New York	29.1	39.6	60.8	66.9	67.5
Queens	25.7	31.0	53.4	61.5	61.6
Richmond	33.2	42.0	53.4	61.1	62.6
Buffalo	25.6	21.3	44.9	46.3	52.0
Rochester	24.7	20.5	41.2	44.8	48.2
Newark	25.0	22.3	41.0	45.6	54.7
Jersey City	36.7	47.0	60.2	71.9	77.7
Philadelphia	21.5	12.1	39.5	42.9	60.5
Pittsburgh	20.1	8.7	42.4	52.9	65.2
Cleveland	30.5	9.1	45.6	50.1	65.4
Detroit	17.6	7.1	36.8	57.2	64.5
Chicago	22.1	20.3	46.5	55.2	62.4
Milwaukee	17.8	9.7	53.7	65.6	74.6
Minneapolis	20.6	6.3	38.8	54.8	58.8
St. Paul	30.8	10.1	51.2	61.2	66.0
Seattle	16.2	6.6	31.9	59.1	66.0
San Francisco	22.1	6.4	49.4	64.8	74.1
Oakland	20.3	6.1	33.6	52.1	63.6
Los Angeles	21.6	7.3	28.7	57.3	67.0

didates for the lesser offices was higher in 1928 than in 1920 or 1924.[12] In Rochester, Jersey City, St. Paul, and three of the counties of New York City, on the other hand, the Democratic vote in 1928 was either lower or not significantly greater than in 1924, and in many of the other areas Democratic increases in 1928 were preceded by gains earlier in the decade and followed by further gains in the 1930's. The data also suggest that in the majority of these urban areas Democratic strength in the 1920's was significantly greater than was indicated by the presidential vote in 1920 and 1924. Several of the areas tended to be predominantly Democratic during much of the 1920's, at

[12] When possible the vote in congressional elections has been displayed in Table II and subsequent tables. Since these metropolitan areas included more than a single congressional district, the congressional vote given is the Democratic percentage of the total vote for candidates for Congress. Thus the influence of the popularity of an individual candidate is somewhat lessened, and the vote presented in this fashion may more nearly reflect underlying partisan divisions. Because of cross-filing and uncontested elections a comprehensive and valid series of congressional returns is, unfortunately, not available for some of these counties. In these cases, the vote for other offices has been substituted.

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least in so far as the vote in elections to lesser offices was concerned. In Boston and Jersey City and in the counties of New York City the Democratic party was apparently the majority party in elections after 1920, and in most areas Democratic candidates for these lesser offices ran well ahead of the Democratic contenders for the presidency in both 1920 and 1924. (See Tables I and II.) Thus shifts toward the Democratic party in 1928 appear much more moderate when the vote in elections to lesser offices is considered.

In still other respects the data displayed in Table II do not provide con-

Table II
Democratic Percentage of the Total Vote for Candidates for Congress in Selected Major Metropolitan Areas, 1920–1936[13]

	Presidential Year Elections					Off Year Elections			
	1920	1924	1928	1932	1936	1922	1926	1930	1934
Boston[a]	39.7	57.2	65.4	68.7	62.1	62.9	54.8	66.1	65.4
Providence	36.8	43.6	51.6	58.0	50.7	55.1	44.7	50.9	59.4
New York City									
Bronx	37.0	56.6	67.1	63.6[b]	75.4	57.9	65.1	68.0	68.6[b]
Kings	36.8	53.7	59.4	64.9	71.4	56.2	61.7	59.5	65.8[b]
New York[a]	58.2	70.1	61.0	70.9	61.3	69.6	69.5	67.6	69.1
Queens	45.1	56.9	57.2	63.4	65.0	70.0[a]	63.4	62.8	66.0
Richmond	44.5	63.1	62.0	66.0	65.5	67.7	67.8	67.2	56.4
Buffalo	35.7	32.8	42.6	48.7	46.8	50.3[a]	38.9	45.7	53.5
Rochester	24.1	41.7[a]	34.1	41.7	50.7	46.1[a]	44.5	42.2	53.5
Newark	32.5	30.2	40.3	44.8	50.6	50.8[c]	37.8	36.5	46.2
Jersey City	48.6	59.3	61.0	70.3	75.1	74.5[c]	76.8	70.5	73.2
Philadelphia	21.4	12.8	38.2	40.2	58.1	18.5	11.5	20.9	48.3
Pittsburgh	19.0	12.2	38.1	44.3	61.6	25.8	11.9	15.1	53.2
Cleveland[a]	49.7	41.9	47.9	50.2	48.1	48.8	48.0	55.3	55.8
Detroit	22.1	18.9	34.7	51.7	59.6	40.8	30.6	20.0	54.6
Chicago	27.9	29.5	47.5	54.9	59.7	46.3	43.4	54.7	61.9
Milwaukee	29.6[a]	17.9	27.7	47.2	44.3	[d]	10.9	16.2	36.5
Minneapolis	9.5	8.4	23.8	23.2[b]	11.6	39.8	8.3	2.6[a]	22.5
St. Paul	34.2	36.9	28.6	24.9[b]	22.9	35.6	3.2	9.0	23.4
Seattle[e]	12.8	20.4	32.2	54.5	62.4	26.3	49.0	40.4	55.9
San Francisco[f]	50.1	[d]	17.9	32.4	[d]	53.3	38.1	10.0	38.9
Oakland[f]	41.0	[d]	17.5	41.0	[d]	33.9	26.5	14.1	37.7
Los Angeles[f]	36.4	[d]	18.3	44.1	[d]	29.2	17.2	34.0	42.0

[13] In this table the letter designations have the following meanings: a, vote for governor substituted; b, at large; c, vote for senator substituted; d, no meaningful returns available; e, vote for first congressional district substituted, 1920–1930; f, vote for senator, 1920, 1928, and 1932, and for governor, 1922, 1926, 1930, and 1934 substituted.

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sistent support for the view that major realignment took place in 1928. If political realignment occurred in 1928 in these areas the vote for lesser offices should show two characteristics: the vote for Democratic candidates should have been significantly higher in 1928 than in 1920 and 1924, and, more importantly, the Democratic vote in 1930 should have remained at higher levels than in 1926. If 1928 marked a lasting partisan realignment rather than a temporary fluctuation in the Democratic vote resulting from short-term factors, then the vote in 1930 should reflect the persistence of this realignment when factors idiosyncratic to 1928 were no longer present. Even this test is, of course, clouded by economic depression. It is possible that increases in the Democratic vote in 1930 as compared with 1926 reflected the impact of the depression that began in 1929 rather than the lasting effects of Smith's candidacy.

In these terms, Table II suggests that lasting partisan realignment may have taken place in some of the metropolitan areas considered. The Democratic vote in Boston, Philadelphia, and Chicago increased by approximately 10 per cent between 1926 and 1930, and more moderate shifts toward the Democratic party apparently occurred in Providence, the Bronx, Buffalo, Pittsburgh, Cleveland, Milwaukee, and St. Paul. But despite these gains, the 1930 Democratic vote in four of these cities (Providence, Buffalo, Pittsburgh, and St. Paul) remained at approximately the same level or below that of 1922, and in St. Paul the Democratic vote in 1928 did not increase, but fell below the levels of 1920 and 1924. Shifts in the Democratic vote in the remaining areas provide even less evidence of major partisan realignment in 1928. In several of these areas the Democratic vote in 1930 was lower than in 1926 or 1922, while in others the vote in 1930 was approximately the same as in the earlier mid-term elections. Because of cross-filing and uncontested elections the returns for the far western metropolitan areas of Seattle, San Francisco, Oakland, and Los Angeles are quite inadequate, but such returns as are available suggest that in the California areas the Democratic vote declined in 1928. Only in the case of Los Angeles was the vote higher in 1930 than earlier in the decade. As in the case of St. Paul, it is difficult to associate this 1930 increase with Smith's candidacy in view of the apparent decline in the Los Angeles Democratic vote in 1928. The very low Democratic vote in 1930 in many of these urban areas, including St. Paul, Minneapolis, Milwaukee, and Pittsburgh, suggests, moreover, that whatever happened in 1928 was not significant enough to make the Democratic party a powerful political force. In some of the metropolitan areas considered, then, the Democratic party was apparently stronger in 1930 than it had been during the earlier years of the 1920's. Whether even these gains can be attributed solely to Smith's candidacy is,

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of course, debatable for it is possible that gains registered by the Democrats in 1930 came in part as a response to economic collapse and had little to do with the presidential election of 1928.

The vote for lesser offices also provides a means to assess the view that in 1928 lasting political realignment came about in part as a result of the movement into the Democratic camp of large numbers of voters who had failed to participate in earlier elections. Examination of the total vote for candidates for lesser offices in these years suggests that in the metropolitan areas considered more voters participated in the election of 1928 than in any election earlier in the decade. Indeed, in several areas the total vote was higher in 1928 than in any election prior to 1936. Many of these urban areas were characterized, however, by rapid population growth in the 1920's, and it is obvious that increases in the total vote were in some measure a reflection of population expansion as well as an indication of changes in patterns of political participation. It is by no means certain, furthermore, that increases in voter participation associated with the 1928 election were solely beneficial to the Democrats; nor is it clear that heavier voter turnout in 1928 marked a lasting increase in voter participation or in either Democratic or Republican strength.

Comparison of the percentage of estimated eligible voters who cast their votes in the mid-term elections of the period provides a basis for at least a suggestive assessment of the impact of the election of 1928 upon patterns of political participation. Again, if the election of 1928 brought about a lasting change in the habits of political participation of significant numbers of former nonvoters, then it could be expected that the rate of voter participation in 1930 would have been higher than in 1926. Table III suggests that in half of the areas voter turnout was at least moderately higher in 1930 than in 1926.[14] In Providence and Cleveland increases in voter turnout were particularly sharp, while in several of the other cities the vote in 1930, although higher than in 1926, was at approximately the same level as in 1922. In the remaining half of the areas, the level of participation in 1930 was approximately the same or lower than in the mid-term elections of the 1920's. The estimates upon which Table III is based are by no means precise enough to lend confidence to conclusions based upon small differences, but it appears that on the whole increases in voter participation in mid-term elections were relatively slight prior to 1934 and that participation increased more con-

[14] For Tables III and IV the estimates of eligible voters, defined as adults of twenty-one and over, are based upon the assumption that the rate of change in the number of persons in this age category was even across the decades of the 1920's and 1930's. Thus the net change in population in this category between the censuses of 1920 and 1930 and 1930 and 1940 has simply been parceled out evenly across the respective decades.

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sistently and more pronouncedly in the earlier 1930's than between 1926 and 1930. In Providence and Cleveland, however, participation increased more sharply between 1926 and 1930 than between 1930 and 1934. In the other metropolitan areas considered, increases in turnout reflected in the elections of the earlier 1930's were of the same magnitude and in many cases were greater than were those that could be associated with the election of 1928.

Table III
Percentage of Estimated Eligible Voters Who Voted for Candidates for Congress in Selected Major Metropolitan Areas, 1922–1934[15]

	1922	1926	1930	1934
Boston[a]	35	36	41	51
Providence	40	40	51	54
New York City				
Bronx	31	31	30	35[b]
Kings	30	30	31	35[b]
New York[a]	27	27	28	33
Queens	33[a]	36	34	37
Richmond	37	41	42	48
Buffalo	35[a]	37	36	49
Rochester	43[a]	45	44	54
Newark	29[c]	21	28	36
Jersey City	46[c]	42	44	54
Philadelphia	27	31	36	52
Pittsburgh	18	21	25	44
Cleveland[a]	25	21	36	43
Detroit	13	12	18	29
Chicago	36	33	37	49
Milwaukee	[d]	23	27	38
Minneapolis	42	33	42[a]	55
St. Paul	35	36	40	53
San Francisco[a]	34	29	32	47
Oakland[a]	39	43	41	56
Los Angeles[a]	31	32	32	56
Seattle[d]				

Although comparison of the vote in 1928 with elections earlier in the decade reveals a substantial increase in Democratic turnout in 1928, it is not certain that this increase was lasting. It is possible that these gains were primarily a response to Smith's personal appeal without permanent significance

[15] In this table the letter designations have the following meanings: a, vote for governor substituted; b, at large; c, vote for senator substituted; d, meaningful returns not available.

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in terms of the partisan identification or habits of political participation of the voters involved. It may be that many individuals who shifted into the Democratic column in 1928 returned to the Republican fold or to their former nonvoting habits in later elections when Smith and other factors peculiar to the election of 1928 were no longer present. By comparing Democratic and Republican turnout in 1930 with 1922 and 1926 some indication can be gained of the impact of increases in voter participation in 1928 upon the relative strength of the two parties. Comparison of these elections (Table IV) suggests that in only five of the metropolitan areas, Boston, Providence, Philadelphia, Cleveland, and Chicago, can increases in voter turnout in these years be construed as working disproportionately to the advantage of the Democrats. In the remaining areas, despite changes in turnout in the 1928 election, the relative balance of Democratic and Republican voters in 1930 was approximately the same as in 1922 and 1926 or had shifted to the advantage of the Republicans. These comparisons, then, provide little support for the view that lasting political realignment occurred in 1928 because of the movement of large numbers of former nonvoters into the Democratic camp.[16] Indeed, the data presented in Table IV are at least equally compati-

Table IV
Percentage of Estimated Eligible Voters Who Voted for Democratic and Republican Candidates for Congress in Selected Major Metropolitan Areas, 1922–1934[17]

		1922	1926	1930	1934
Boston[a]	D	22	20	27	33
	R	12	16	13	15
Providence	D	22	18	26	32
	R	18	22	25	22
New York City					
Bronx	D	18	20	20	24[b]
	R	9	8	6	6
Kings	D	17	18	18	23[b]
	R	11	10	8	8

[16] Observations as to the impact of new voters upon the relative strength of the two parties are subject to a possible fallacy. Increases in the party vote in particular years as reflected in Table IV are, obviously, composed of two elements: voters who had voted in earlier elections and who switched to the opposite party, and voters who had not voted in earlier elections. The relative importance of these two components in the shifts in electoral strength cannot, of course, be determined. Thus it is possible to assume, for example, that the total Republican increase in all areas or in any individual area was disproportionately composed of old voters who switched to the Republicans in 1928 while the Democrats received a heavily disproportionate share of the new voters.

[17] In this table the letter designations have the following meanings: a, vote for governor substituted; b, at large; c, vote for senator substituted; d, no meaningful returns available.

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Table IV (*continued*)

		1922	1926	1930	1934
New York[a]	D	19	19	19	23
	R	7	7	7	8
Queens	D	23[a]	23	22	25
	R	9	13	11	11
Richmond	D	25	28	28	27
	R	11	13	12	13
Buffalo	D	18[a]	15	16	26
	R	14	22	18	21
Rochester	D	20[a]	20	18	29
	R	20	23	24	23
Newark	D	15[c]	8	10	17
	R	14	13	17	19
Jersey City	D	34[c]	32	31	40
	R	11	10	13	14
Philadelphia	D	5	4	8	25
	R	21	27	28	26
Pittsburgh	D	5	2	4	23
	R	11	17	21	17
Cleveland[a]	D	12	10	20	24
	R	13	11	16	18
Detroit	D	5	4	4	16
	R	8	8	14	12
Chicago	D	17	14	20	30
	R	18	19	17	19
Milwaukee	D	[a]	3	4	14
	R	[d]	11	12	9
Minneapolis	D	17	3	1[a]	12
	R	25	21	17	18
St. Paul	D	12	1	4	12
	R	21	13	26	19
San Francisco[a]	D	18	11	3	18
	R	15	17	28	24
Oakland[a]	D	13	11	6	21
	R	24	30	33	30
Los Angeles[a]	D	9	5	11	23
	R	20	25	20	26
Seattle[d]					

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ble with the view that shifts in Democratic turnout and in the Democratic share of the total vote in 1928 were of an essentially short-term nature and did not mark a lasting change in the habits of political participation or partisan loyalties of any significant segment of the electorate.

In many of these areas shifts toward the Democratic party were greater in the earlier 1930's than were those associated with the election of 1928 and that might be attributed to the influence of the Democratic candidate in that year. (See Table IV.) In the metropolitan areas of Boston, Providence, Cleveland, and Chicago increases in Democratic strength between 1926 and 1930 were of approximately the same magnitude or were greater than the shifts toward the Democrats in the 1930's. In the other areas, however, the increases in Democratic strength between the elections of 1930 and 1934 were greater than were those that could be attributed to changes in patterns of political behavior resulting from the election of 1928.

In short, examination of the vote in elections to lesser offices in metropolitan counties characterized by a population composed of large immigrant stock does not provide consistent support for the view that significant and lasting changes in partisan loyalties and habits of political participation occurred in 1928. The pattern of the vote in several areas, including Boston, Providence, Chicago, Cleveland, and Philadelphia, is at least compatible with that view, although in the last city the Democratic party remained in 1930 a relatively insignificant political alternative. In several other areas the Democratic party enjoyed very small increases perhaps as a result of the Smith candidacy. Whether these gains were of sufficient magnitude to merit characterization as revolution is another matter, and in these as in most of the areas considered the gains of 1928 were followed by more pronounced increases in the elections of the earlier 1930's. Thus, the pattern of the vote in the majority of these metropolitan areas either provides no support for the view that an "Al Smith Revolution" preceded the "Roosevelt Revolution" of the 1930's or is at most inconclusive.

On the basis of the presidential vote alone it could be concluded that in these areas major and lasting partisan realignment occurred in 1928, and it is also possible to conclude that these changes came about in large measure because of the characteristics of Alfred E. Smith and the existing social order. But when the vote in elections to lesser offices is considered, this general interpretation of the electoral history of the 1920's and 1930's appears much less tenable. Indeed, shifts in this vote seem equally consistent with the view that realignment came primarily in the 1930's or with the suggestion that

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realignment came about in the course of a "critical period" rather than as a result of a single "critical election."

The present examination does not, of course, prove that partisan realignment did not occur in 1928 in these or other areas. Detailed investigation of a larger number of areas employing powerful methods of analysis and taking into consideration, perhaps, the vote for additional offices might provide evidence of major realignment in that year. The areas considered are, moreover, of large size, and it is possible that voting patterns at the county level conceal rather than reveal significant trends and changes. Indeed, examination of the vote in individual congressional districts suggests that changes in the vote in several areas were not evenly distributed, but instead tended to be concentrated in particular districts. Thus it is quite conceivable that investigation of voting patterns at the level of wards and precincts would reveal evidence of lasting partisan change in 1928. Such an analysis might indicate that some immigrant groups defined by religious, economic, and more specific ethnic characteristics did change meaningfully in political behavior while others did not. It may be significant, however, that these shifts, if they occurred, were apparently not of a magnitude sufficient to alter greatly the balance of party strength within most of the metropolitan areas considered here. From a somewhat different point of view, changes in political behavior in 1928 even though not lasting in terms of the election of 1930 may have been a significant or event necessary step in the process of partisan realignment. Deviations in 1928 on the part of some groups from established patterns of partisan identification and political behavior may have increased the likelihood that a more lasting change in the political behavior of these same groups would occur at a later time.[18]

It is clear, however, that the question of whether major partisan realignment occurred in 1928 can be answered, if it can be answered at all, only through detailed investigation of the voting patterns of 1928 and of earlier and subsequent elections, including the vote for lesser offices as well as the presidential vote.[19] It may be tempting to believe that the impact of Alfred E. Smith upon the electoral patterns of the nation can be established on the

[18] This possibility is raised in Campbell, Converse, Miller, and Stokes, *Elections and the Political Order*, 75.

[19] This and related imperatives for the study of political history are discussed in detail in Lee Benson, "Research Problems in American Political Historiography," in *Common Frontiers of the Social Sciences*, ed. Mirra Komarovsky (Glencoe, Ill., 1957), 113–81. It is probable that students of American electoral patterns have based their investigations almost exclusively upon the presidential vote primarily because returns for elections to other offices were not available. County-level returns for elections to the offices of governor, senator, and representative in Congress, as well as presidential returns, for the years from 1824 to the present can now, however, be supplied on a limited basis by the Inter-university Consortium for Political Research, Box 1248, Ann Arbor, Michigan, 48106.

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basis of other forms of evidence. The thought that Al Smith, the rags-to-riches scion of the Fulton Fishmarket, was responsible for bringing the children of the "new immigration" into an increasingly welfare-oriented Democratic party is in many respects an appealing one. Perhaps in some sense that is what should have happened. That it did happen can be established only through detailed analysis of the actual voting record.

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The Study of United States History in the Soviet Union

N. N. Bolkhovitinov
Translated by Marin Pundeff

AMERICAN studies in the Soviet Union are a comparatively young branch of historical science. They not only lack deep traditions in prerevolutionary historiography and such brilliant predecessors as N. I. Kareev, I. V. Luchitskii, M. M. Kovalevskii, and, somewhat later, the Academician E. V. Tarle (the famous Russian school of the study of the history of France), but even in the Soviet era their appearance was rather belated. As a result, one may justifiably speak of American studies as an independent field in the Soviet Union only in the 1950's and 1960's, although the first serious investigations in the history of the United States appeared in the mid-1930's.

In considering the origins of the study of US history in the Soviet Union, one must first mention the names of the Corresponding Member of the Academy of Sciences, A. V. Efimov, and Professor L. I. Zubok. The first monographic research was Efimov's doctoral dissertation, *A Contribution to the History of Capitalism in the USA* (1934); it was destined to play a leading role in the development of American studies. It is no exaggeration to say that an entire generation of Soviet historians specializing in US history was brought up on this book. It is regrettable that it has not yet been republished and has long been a bibliographic rarity. Having studied a considerable mass of sources and writings, Efimov was able, with his typical aptitude for broad generalizations, to raise important theoretical questions concerning the development of capitalism in the US: the problem of the so-called "free lands"; the nature of American slavery; and the basic stages and special features of the Industrial Revolution. In his conclusions the author persuasively proved that the development of the US proceeded in conformity with the general laws of capitalist society discovered by Karl Marx and Friedrich Engels, and he generalized that there are no reasons "for assertion of some 'exceptional nature' of American capitalism."[1] Though Efimov considered

►*Mr. Bolkhovitinov, of the Institute of History of the Academy of Sciences of the USSR, presented this paper in a series of lectures in the United States during the summer of 1968.*

[1] A. V. Efimov, *K istorii kapitalizma v SShA* [A Contribution to the History of Capitalism in the USA] (Moscow, 1934), 246.

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some of the most important problems of the historical development of the United States, he did not claim in any measure to have solved fully and definitively all questions raised in his monograph; in his own view, it had the nature of "a preliminary investigation."[2]

A major part in the preparation of cadres of Soviet Americanists was subsequently played by the original courses of lectures of Efimov and Zubok, published in the mid-1950's as *Outlines of the History of the USA*.[3] Principal attention in these works was devoted to socioeconomic problems, elucidation of general as well as specific features of American capitalism, the class struggle, the labor and Negro movements, and the unmasking of the expansionist foreign policy of the ruling circles of the United States in the nineteenth and the beginning of the twentieth centuries. These traditions were developed in the new works of Soviet historians, particularly in the two-volume synthesizing work on the history of the US[4] and the numerous special studies on general and specific problems of American history. Though the Soviet works differ in the variety of style, depth of investigation, breadth of coverage of documentary material, and level of professionalism, they have a common set of features: the Marxist method of analysis, attention to the study of the role in the history of the US of its real makers—the workers, farmers, and artisans—the study of the struggle of the Negro people for its liberation, and the unmasking of the reactionary domestic and foreign policy of the ruling circles in Washington.

The development of American studies, as that of our historical science in general, was, of course, far from smooth. Until the mid-1950's the major investigations on the history of the US could literally be counted on one's fingers. A lack of trained cadres of specialists and the influence of the cult of the personality of Stalin contributed to the problem. A portion of what was written (concerning mainly the problems of foreign policy of the most recent period) has lost its former "topical interest" and has become hopelessly obsolete. For many periods of American history there are still gaps. Many major problems have remained not only unsolved but even untouched. Even a cursory examination of the literature on the earliest, colonial period and the problem of the genesis of American capitalism connected with it makes this evident. The only serious monographic work dealing with the initial stage of the history of the US remains the book by Professor A. S.

[2] *Id., Ocherki istorii SShA. Ot otkrytiia Ameriki do okonchaniia grazhdanskoi voiny* [Outlines of the History of the USA. From the Discovery of America to the End of the Civil War] (2d ed., Moscow, 1958), 432.

[3] *Ibid.* (Moscow, 1955); L. I. Zubok, *Ocherki istorii SShA (1877–1918)* [Outlines of the History of the USA (1877–1918)] (Moscow, 1956).

[4] *Ocherki novoi i noveishei istorii SShA dvukh tomakh* [Outlines of the Modern and Contemporary History of the USA in Two Volumes] (Moscow, 1960).

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Samoilo, *English Colonies in North America in the 17th Century*. Skillfully using English and American sources, the author painted a detailed picture of English colonization of the North American continent and devoted special attention to the attempts of the mother country to implant feudal relations in the colonies.[5] Despite the value of Samoilo's investigation, the questions of the genesis of capitalism and, in particular, the problem of the initial accumulation of capital in America have remained outside the scope of his attention. One of Samoilo's students, V. F. Stratanovich, wrote a dissertation on the genesis of American capitalism in the seventeenth and eighteenth centuries and published several articles in the *Uchenye Zapiski* of the N. K. Krupskaia Pedagogical Institute of Moscow (including an article on the initial accumulation of capital in North America),[6] but the scholarly problem of the genesis of the capitalist mode of production, and especially of the initial accumulation of capital, has remained unsolved. We may recall that, in conformity with the classic formulation of Marx, "the initial accumulation of capital is nothing but the process of separation of the producer from the means of production."[7] There is no simple solution as to how this process took place in the territory of North America. Samoilo called the seizure of the lands of the Indians "the American variant of the plunder of the peasants," carried out in England by enclosures, but it cannot be denied that the results of these two processes turned out to be entirely different.[8] This, of course, was right. The Indians were in fact driven out of the lands belonging to them and even exterminated, but they did not become hired laborers.

Stratanovich has studied in great detail "the main sources of the initial accumulation of capital," including the "plunder of the indigenous population," the "cruel exploitation of the so-called white slaves and the working population of the colonies," the plantation slavery of the Negroes, the slave trade, and so on, but in essence he said nothing about the principal source: the formation of hired labor.[9]

The failure to explain the problem of initial accumulation of capital in North America, is, in my opinion, to be explained above all by the fact that the investigators have looked in the colonies for a process that had not actually

[5] See A. S. Samoilo, *Angliiskie kolonii v Severnoi Armerike v* XVII *veke* [English Colonies in North America in the 17th Century] (Moscow, 1963), 211–92.

[6] See V. F. Stratanovich, "K voprosu o genezise kapitalizma v angliiskikh koloniiakh Severnoi Ameriki v XVII–XVIII vv." [On the Question of the Genesis of Capitalism in the English Colonies in North America in the 17th and 18th Centuries], *Uchenye Zapiski MOPI im. N. K. Krupskoi. Istoricheskii fakul'tet*, CLIX (No. 6); CLXXIV (No. 7).

[7] Karl Marx and Friedrich Engels, *Sochineniia* [Works], XXIII, 727.

[8] Samoilo, *Angliiskie kolonii*, 312.

[9] V. F. Stratanovich, "K voprosu o pervonachal'nom nakoplenii kapitala v angliiskikh koloniiakh v Severnoi Amerike v XVII–XVIII vv." [On the Question of the Initial Accumulation of Capital in the English Colonies in North America in the 17th and 18th Centuries], *Uchenye Zapiski MOPI im. N. K. Krupskoi. Istoricheskii fakul'tet*, CLIX, 184.

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taken place there. Thus far not a single investigator has succeeded in adducing facts (even isolated ones) concerning a forcible separation of producers from the means of production in colonial America and their transformation into hired laborers. In one way or another, the colonies were settled, and the settlers, as a rule, were not deprived of land, but, on the contrary, acquired it in various ways. Thus, the principal source of the initial accumulation of capital, we suspect, should be sought in the mother country, not in the colonies. The metropolitan areas, Western Europe and, above all, England, served precisely as the principal source of the labor force for developing American capitalism in the colonial period, throughout the entire nineteenth century, and partially in the twentieth century. The key to solving the problem is thus in the intimate connection of the genesis of capitalism in the mother country with the establishment of capitalist relations in the colonies, while any isolated consideration of the internal American processes alone impedes and even renders impossible penetration to the core of the problem.[10]

The history of the establishment and development of capitalism in the US involves a whole series of other complex questions such as the nature of plantation slavery, the stages and special features of the Industrial Revolution, the development of capitalism in agriculture, the problem of the "moving frontier" and the so-called "safety valve," and so on. To some extent these problems have already been the subject of special study by Soviet historians, and the process of solving them has brought the first successes. Their definitive and comprehensive investigation, however, still lies ahead. The publication of new works does not, moreover, always advance the solution of the problems involved. Thus, for example, the above-mentioned monograph of Efimov was the first to provide a somewhat detailed analysis of the development of the Industrial Revolution in the US. Not all aspects of this complex process were examined in sufficient detail by the author, and Soviet historians still face the task of studying this problem deeply and comprehensively. Several other investigators have touched the problem, but were unable to solve it.

We already have had to polemicize in connection with the dissertation of B. M. Kosarev, "Plantation Slavery of the Negroes in the US and Its Characterization in the Works of Marx and Engels."[11] It should be noted that at first Kosarev placed the origin of the Industrial Revolution in the US

[10] The three-volume work that is being prepared by the Institute of History of the USSR Academy of Sciences under the editorship of Academician S. D. Skazkin, "The Genesis of Capitalism in the Countries of Europe, Asia, and America," is intended to solve this problem.

[11] See the section "Further Discussion of the Nature of American Slavery in the Nineteenth Century," in N. N. Bolkhovitinov, *Doktrina Monro (proizkhozhdenie i kharakter)* [The Monroe Doctrine (Origin and Nature)] (Moscow, 1959), 21–24.

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in the second half of the 1840's, in this respect basing his interpretation on Lenin's observation that America began its "industrial" development at that time.[12] It is known, however, that the Industrial Revolution and industrialization are not equivalent concepts. The Industrial Revolution precedes capitalist industrialization and is, actually, its point of departure. Later Kosarev began to retreat from his initial point of view; not long ago he published an article in which he proposed that the origin of the Industrial Revolution in the US be placed in the 1820's.[13] At the same time, to prove this point, the author essentially produced no data that would show any qualitative changes in the development of American industry in the 1820's. American industry did, of course, register certain gains in these years, but they were not as significant as those in 1812–1815 or at the end of the 1830's and the beginning of the 1840's, when the decisive stage of the Industrial Revolution began. Kosarev does refer to "qualitative changes in the sphere of social relations" and notes that the movement of "hired labor" in the US began in the second half of the 1820's.[14] The Industrial Revolution is, of course, inseparable from the qualitative changes in the social sphere, but these changes occur not before or even contemporaneously with the beginning of the technological revolution but after it has begun and as a result of this revolution.

I should like also to make a general observation regarding the considerable number of so-called "preliminary" and "announcing" publications. As I see it, a printed work must be the completion of a comprehensive and thorough study of the problem, whereas the requirement of preliminary publication of the basic conclusions of a dissertation often serves as an excuse to publish raw and unfinished investigations. Only in exceptional cases is the investigator justified in "staking out" the preliminary results of his work: when a major scientific discovery is involved, something that does not happen frequently in contemporary science (and especially in history). It is not always necessary to publish even completed dissertations, especially if they do not contribute anything basically new to the problem investigated.

After these critical remarks I would like to draw attention to one of the best articles on the history of the genesis of capitalism: the work of G. P. Kuropiatnik on the capitalist evolution of agriculture in the eighteenth and

[12] B. M. Kosarev, "K voprosu o vozniknoveniia i razvitii plantatsionnogo rabstva v SShA" [On the Question of the Origin and Evolution of Plantation Slavery in the USA], *Uchenye Zapiski Kostromskogo pedagogicheskogo instituta* (No. 2, 1957), 29, referring to V. I. Lenin, *Sochineniia* [Works], XIII, 292.

[13] B. M. Kosarev, "O nachale promyshlennogo perevorota v SShA" [On the Beginning of the Industrial Revolution in the USA], *Iaroslavskii gosudarstvennyi pedagogicheskii institut im. K. D. Ushinskogo. Uchenye Zapiski* (No. 58, 1966), 99–125.

[14] *Ibid.*, 103, 112–13.

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nineteenth centuries. Basing his analysis on censuses, the investigator reached the important conclusion that after the Civil War, although there was a strong trend toward the "Prussian" way, in the South as a whole the American, farmer way of the development of capitalism in agriculture prevailed. "The basis for the development of capitalism became not the landlord estates of the former slaveowners worked by sharecroppers, but the mass of independent small farms free from feudal survivals." By 1900 the number of farmers who owned their land amounted to more than 63 per cent of the total value of agricultural production in the South.[15]

On a country-wide scale, the author writes in conclusion, the economic basis for the farmer-type of capitalistic development in agriculture was created by means of squatter settlement; confiscation of the estates of the landed aristocracy during the War of Independence; nationalization of the western lands; agrarian legislation on distribution of western lands during the first half of the nineteenth century; destruction of the slave-owning estates of the great plantation owners during the Civil War; and adoption of the Homestead Act for free distribution of western lands.[16]

It should be recalled that the bases of the Marxist analysis of capitalistic development in American agriculture were laid by Lenin himself, who made a brilliant, and in time classic, analysis of materials of the censuses of 1900 and 1910.[17] In the mid-1930's Lenin's work was supplemented by materials relating to the later period.[18] Investigation of nineteenth-century censuses would make possible the detailed tracing of the roots of the phenomena that became the object of Lenin's careful attention at the beginning of the twentieth century. Soviet historians still have to perform this task, but the first steps in that direction have already been taken. The study of the development of capitalism in agriculture and the farmer movement in the last third of the nineteenth century continues to preoccupy Kuropiatnik.[19] The

[15] G. P. Kuropiatnik, "O puti razvitiia kapitalizma v zemledeliia SShA v domonopolistiicheskuiu epokhu" [On the Way of the Development of Capitalism in the Agriculture of the USA in the Premonopolist Period], *Novaia i Noveishaia Istoriia* (No. 4, 1958), 49–50.

[16] *Ibid.*, 55.

[17] V. I. Lenin, "Novye dannye o razvitii kapitalizma v zemledelie Vypusk 1. Kapitalizm i zemledelie v Soedinennykn Shtatakh Ameriki" [New Data on the Development of Capitalism in Agriculture. Section 1. Capitalism and Agriculture in the United States of America], *Polnoe sobranie sochinenii* [Complete Collection of Works], XXX.

[18] *Novye materialy k rabote V. I. Lenina "Novye dannye o zakonakh razvitiia kapitalizma v zemledelii. Vypusk 1. Kapitalizm i zemledelie v Soedinennykh Shtatakh Ameriki"* [New Materials on the Work of V. I. Lenin, "New Data on the Laws of the Development of Capitalism in Agriculture. Section 1. Capitalism and Agriculture in the United States of America"], ed. E. S. Varga (Moscow, 1937).

[19] G. P. Kuropiatnik, "Fermerskoe dvizhenie v Soedinennykh Shtatakh Ameriki v 40-kh godakh XIX veka" [The Farmer Movement in the United States of America in the 1840's], *Uchenye Zapiski po Novoi i Noveishei Istorii* (No. 2, 1956), and "Zakon o gomstedakh i 'Amerikanskii' put' razvitiia kapitalizma v sel'skom khoziastve" [The Homestead Act and "the

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question of the use of government lands prior to 1860 is investigated by M. V. Demikhovskii of Baku, who has already published fragments of his work in historical journals.[20] Soviet historians have devoted much attention to the role of western lands in the history of the US, to the critical analysis of the conception of F. J. Turner and his school, and to the question of the so-called "safety valve."[21]

There are many gaps in the study of the political history of the United States. It is true that a general book by V. I. Lan on classes and parties in the US in the nineteenth and twentieth centuries[22] appeared in the 1930's, but subsequently these topics (at any rate, for the period of recent history) have been the subject of no special investigations.[23] Only recently have dissertations been written on such important problems as "Jacksonian democracy" (M. N. Mezhevich of Chernovtsy) and the "antitrust" policy of Theodore Roosevelt (A. V. Dmitriev of Leningrad).[24] One awaits with great interest the appearance of the doctoral dissertation of I. A. Beliavskaia, "Bourgeois Reformism in the US in the Beginning of the Twentieth Century," fragments of which have already been published as separate articles.[25] Beliavskaia wrote one of the few special studies in which, on the basis of a

American Way" of the Development of Capitalism in Agriculture], in *K stoletiiu grazhdanskoi voiny v SShA* [On the Centennial of the Civil War in the USA], ed. A. V. Efimov and L. I. Zubok (Moscow, 1961).

[20] M. V. Demikhovskii, "Bor'ba za zemli tak nazyvaemogo obshchestvennogo fonda SShA v 20–30-kh godakh XIX veka" [The Struggle for the Lands of the So-Called Public Pool of the USA in the 1820's and 1830's], *Novaia i Noveishaia Istoriia* (No. 1, 1962), and "Politika praviashchikh krugov SShA v otnoshenii indeiskogo naseleniia" [The Policy of the Ruling Circles of the USA toward the Indian Population], *Voprosy istorii* (No. 2, 1966).

[21] A. V. Efimov, "Problema sosushchestvovaniia razlichnykh sposobov proizvodstva v rabotakh F. D. Ternera" [The Problem of the Coexistence of Various Methods of Production in the Works of F. J. Turner], *Voprosy Filosofii* (No. 5, 1956); N. N. Bolkhovitinov, "O roli 'podvizhnoi granitsi' v istorii SShA (Kriticheskiieanaliz konts ptsii F. D. Ternera)" [On the Role of the "Moving Frontier" in the History of the USA (A Critical Analysis of F. J. Turner's Conception)], *Voprosy istorii* (No. 9, 1962).

[22] V. I. Lan, *Klassy i partii v SShA. Ocherki po ekonomicheskoi i politicheskoi istorii SShA* [Classes and Parties in the USA. Outlines of the Economic and Political History of the USA] (Moscow, 1932; 2d ed., 1937).

[23] The period of recent history is dealt with in very small sections of the books of Anatolii Gromyko, *Kongress SShA* [The Congress of the USA] (Moscow, 1957); E. B. Cherniak, *Gosudarstvennyi stroi i politicheskie partii SShA* [The Government System and Political Parties of the USA] (Moscow, 1957); G. G. Boichenko, *Konstitutsiia Soedinennykh Shtatov Ameriki* [The Constitution of the United States of America] (Moscow, 1959).

[24] M. N. Mezhevich, *Politicheskaia bor'ba v SShA v 30-kh godakh* XIX *veka* [The Political Struggle in the USA in the 1830's] (Chernovtsy, 1963); A. V. Dmitriev, *"Antitrestovskaia" politika amerikanskogo pravitel'stva (1901–1908)* [The "Antitrust" Policy of the American Government (1901–1908)] (Leningrad, 1963).

[25] I. A. Beliavskaia, "Imperialisticheskaia sushchnost' antitrestovskogo zakonodatel'stva T. Ruzvel'ta (1900–1905 gg.)" [The Imperialist Nature of the "Antitrust" Legislation of T. Roosevelt (1900–1905)], *Uchenye Zapiski po Novoi i Noveishei Istorii* (1957), and "Bortsy protiv zakonov 'dzhunglei' (O dvizhenii 'razgrebatelei griazi' v SShA v 1900-e gody)" [Fighters against the Laws of the "Jungle" (On the Movement of the "Muckrakers" in the USA in the 1900's)], *Novaia i Noveishaia Istoriia* (No. 4, 1967).

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mass of material, the process of the consolidation of state monopoly tendencies during World War I is traced and the class orientation of President Wilson's policy is demonstrated.[26]

N. M. Gol'dberg of Leningrad has devoted a study to the problem of freethinking and atheism in the US in the eighteenth and nineteenth centuries; in it he has analyzed the views of Benjamin Franklin, Thomas Jefferson, I. Allen, Thomas Paine, T. Cooper, Robert Ingersoll, Frederick Douglass, and others.[27] The history of religion in the US and particularly of the Baptist Church at the beginning of the twentieth century is being studied successfully by A. A. Kislova. She has already written a series of articles on this subject and has prepared a monograph for publication.[28]

A relatively developed area in the domestic history of the US is the labor and socialist movements. The noted Soviet historian Professor Zubok has done the most meritorious work in this area. His monograph, *Outlines of the History of the Labor Movement in the USA (1865–1918),* summarized in a way the author's almost forty years of fruitful work, in the course of which he published numerous articles on widely varying questions in the history of the class struggle of the American proletariat in the nineteenth and twentieth centuries.[29]

It must be said, of course, that some experience in the study of the socialist and labor movements in the US was gained in the 1920's and the early part of the 1930's when the first works on industrial workers throughout the world appeared.[30] Only after the publication of Zubok's general and

[26] *Id., Vnutrenniaia ekonomicheskaia politika SShA (1917–1918)* [The Domestic Economic Policy of the USA (1917–1918)] (Moscow, 1956).

[27] N. M. Gol'dberg, *Svobodomyslie i ateizm v SShA* (XVIII–XIX *vv.*) [Freethinking and Atheism in the USA (18th and 19th Centuries)] (Moscow, 1965).

[28] A. A. Kislova, "K istorii amerikanskogo baptizma" [On the History of the Baptist Church in America], *Voprosy istorii religii i ateizma* (No. 12, 1964), "Voprosy religioznogo modernizma v protestantskoi teologii" [Questions of Religious Modernism in Protestant Theology], *Voprosy istorii* (No. 10, 1966), and "Negry i baptistskaia tserkov'" [Negroes and the Baptist Church], *Nauka i religiia* (No. 11, 1966).

[29] L. I. Zubok, *Ocherki istorii rabochego dvizheniia v SShA (1865–1918)* [Outlines of the History of the Labor Movement in the USA (1865–1918)] (Moscow, 1962). Among Zubok's earlier works one may mention the articles "Lenin ob amerikanskom rabochem dvizhenii" [Lenin on the American Labor Movement], *Bor'ba Klassov* (No. 2, 1934), and "Engel's i amerikanskoe rabochee dvizhenie" [Engels and the American Labor Movement], *Istorik-Marksist* (No. 2, 1936). On Zubok's scholarly activity, see also *Voprosy istorii* (No. 9, 1967), 217.

[30] A. Chekin, *Industrial'nye rabotniki mira (K kharakteristike professional'nogo dvizheniia Soedinennykh Shtatov)* [Industrial Labor throughout the World (Toward the Characterization of the Trade-Union Movement in the United States)] (Moscow, 1921); A. F. Dobrynin, "Promyshlennye rabochie mira" [Industrial Labor of the World], *Zapiski nauchnogo obshchestva marksistov* (No. 5, 1923); Ia. Vilenkin, "De Leon i ego deiatel'nost'" [De Leon and His Activity], *Pod znamtenem marksizma* (No. 3, 1926); L. G. Raiskii, "Rabochee dvizhenie v SShA v kontse XIX i nachale XX vv." [The Labor Movement in the USA at the End of the 19th and the Beginning of the 20th Century], in *Kniga dlia chteniia po istorii novogo i noveishego vremeni* [Readings on the History of the Modern and Contemporary Era] (Kharkov, 1930), III, and *Noveishaia istoriia Severo-Amerikanskikh Soedinennykh Shtatov posle*

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special works can it be said, however, that the study of the labor and socialist movements in the US became somewhat systematic. Zubok's teaching played a significant role in this respect. Though the majority of specialists on the labor movement are presently involved in studying the most recent period, work on the earlier period is also being advanced quite successfully. In the last few years substantial works by B. D. Kozenko of Saratov have appeared; they are written in a lively, literary manner.[31] V. T. Buchin of Leningrad has devoted his *kandidat* dissertation to the Socialist party of the US. Somewhat earlier T. K. Gladkov published a small book on the life and activity of William Haywood.[32] S. M. Askol'dova devoted her monograph to the first nationwide manifestation of the industrial proletariat of the US as a class in the 1880's: the general strike for an eight-hour day in May 1886. In the author's opinion, the vivid events of the 1880's marked a new stage in the struggle of the American proletariat with the bourgeoisie—the beginning of the contemporary labor movement in the United States.[33] A wide range of questions concerning the labor and socialist movements in the nineteenth and the beginning of the twentieth centuries is dealt with in the works of A. A. Poletaev, I. I. Cherkazov, and Z. K. Eggert.[34]

Finally, Soviet investigators have devoted their most serious attention to the problem of immigration and, in particular, to the role of immigration in the formation of the American working class and the study of its influence on the struggle of the workers of the US. The results of these investigations have been synthesized in the comprehensive work of A. N. Shlepakov of Kiev, the first chapters of which deal with the end of the nineteenth and the beginning of the twentieth century.[35] N. A. Erofeev and Sh. A. Bogina have studied the earlier period in great detail.[36]

grazhdanskoi voiny i do mirovoi imperialisticheskoi voiny [The Modern History of the United States of North America from the Civil War to the World Imperialist War] (Leningrad, 1930).

[31] B. D. Kozenko, *Rabochee dvizhenie SShA v gody pervoi mirovoi voiny* [The Labor Movement in the USA in the Years of World War I] (Saratov, 1965), and *Iudzhin Debs, sotsialist* [Eugene Debs, Socialist] (Saratov, 1967).

[32] T. K. Gladkov, *Zhizn' Bol'shogo Billa* [The Life of Big Bill] (Moscow, 1960).

[33] S. M. Askol'dova, *Nachalo massovogo rabochego dvizheniia v SShA (80-e gody XIX v.)* [The Beginnings of the Mass Labor Movement in the USA (1880's)] (Moscow, 1966), 3.

[34] A. A. Poletaev, "Klassovye boi amerikanskogo proletariata v 1876–1877 godakh" [The Class Struggles of the American Proletariat in 1876–1877], *Voprosy istorii* (No. 11, 1949), and "Marks i Engel's o rabochem dvizhenii v SShA v 70–90-kh godakh XIX v." [Marx and Engels on the Labor Movement in the USA in the 1870's to the 1890's], in *Iz istorii bor'by Marksa i Engel'sa za proletarskuiu partiiu* [From the History of the Struggle of Marx and Engels for a Proletarian Party] (Moscow, 1955); I. I. Cherkasov, "Iz istorii rasprostraneniia marksizma v SShA" [From the History of the Dissemination of Marxism in the USA], *Novaia i Noveishaia Istoriia* (No. 3, 1958); Z. K. Eggert, "De Leon—ideolog anarkho-sindikalizma v amerikanskom rabochem dvizhenii" [De Leon—An Ideologist of Anarchosyndicalism in the American Labor Movement], in *Iz istorii sotsial'no-politicheskikh idei* [From the History of Sociopolitical Ideas] (Moscow, 1956).

[35] A. N. Shlepakov, *Immigratsiia i amerikanskii rabochii klass v epokhu imperializma* [Immigration and the American Labor Class in the Age of Imperialism] (Moscow, 1966).

[36] N. A. Erofeev, *Narodnaia emigratsiia i klassovaia bor'ba v Anglii v 1825–1850 gg.*

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One of the most enduring traditions of Soviet historiography is the profound interest in popular movements of the past and above all in social revolutions, the "locomotives of history," in Marx's phrase. It is thus indicative that the first book on the history of the US to appear in the Soviet Union was the work of scholarly popularization by D. O. Zaslavskii on the Civil War. Somewhat later he published an outline on the history of the United States in the eighteenth and nineteenth centuries.[37] Regardless of a certain schematism and brevity in his works, the author was the first to succeed in providing a Marxist outline of the general history of the US, and he did it in an interesting and engrossing way. Zaslavskii was not a professional historian; he was a talented man of letters and a publicist who made a major contribution to the development of Soviet journalism. His books on American history, however, were destined to have a long life. They have been read and reread not only by specialists but by the widest circles of the Soviet intelligentsia. The author concentrated mainly on the Negro problem, the abolitionist movement, and especially the Civil War; to the present these problems still receive the most attention from Soviet Americanists. In recent years alone some twenty major works (monographs and dissertations) have been devoted specifically to problems of the Civil War and Reconstruction (by A. I. Blinov, Sh. A. Bogina, I. P. Dement'ev, M. N. Zakharova, R. F. Ivanov, G. P. Kuropiatnik, I. Ia. Levitas, D. B. Petrov, P. B. Umanskii, and others), not counting a large number of research articles, pamphlets, and surveys. In no other country (with the exception, naturally, of the US) have such a substantial number of special works been published on these problems, and it can be stated without exaggeration that a small but active school of historians specializing in the second American revolution has arisen in the Soviet Union.

In the course of examining Russia's position during the Civil War in the US, this school made its greatest contribution: unearthing for scholarly use materials from the Russian press and archives. Investigation of this question was begun in the 1930's through publication of archival documents edited by E. A. Adamov, A. V. Efimov, and M. M. Malkin.[38] The mono-

[Popular Emigration and the Class Struggle in England in 1825–1850] (Moscow, 1962); Sh. A. Bogina, *Immigratsiia v SShA nakanune i v period grazhdanskoi voiny (1850–1865)* [Immigration into the USA on the Eve and during the Civil War (1850–1865)] (Moscow, 1965). The book of L. A. Bagramov, *Immigranty v SShA* [Immigrants in the USA] (Moscow, 1957), is also devoted to the problem of immigration as a whole.

[37] D. O. Zaslavskii, *Grazhdanskaia voina v Soedinennykh Shtatakh Severnoi Ameriki (1861–1865)* [The Civil War in the United States of North America (1861–1865)] (Leningrad, 1926), and *Ocherki istorii Severo-Amerikanskikh Soedinennykh Shtatov* XVIII–XIX *vv.* [Outlines of the History of the United States of North America in the 18th and 19th Centuries] (Moscow, 1931).

[38] E. A. Adamov, "Soedinennye Shtaty v epokhu grazhdanskoi voiny i Rossiia" [The United States in the Years of the Civil War and Russia], *Krasnyi Arkhiv,* XXXVIII (1930);

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graph of the Leningrad researcher, Malkin, *The Civil War in the USA and Tsarist Russia,* also continues to have scholarly significance. This monograph, based on a wide use of sources and writings, provided for the first time a comprehensive analysis of the position of tsarist diplomacy and a detailed analysis of the basic problems of the domestic history of the Civil War and of international relations of the period. It can only be regretted that this book was not translated into English; in the preface Academician E. V. Tarle recommended this.[39]

Many Soviet authors have subsequently touched upon the subjects with which Malkin's monograph dealt. I. I. Liagushchenko of Saransk prepared a special dissertation on the views of N. G. Chernyshevskii regarding the Civil War; I. Ia. Razumnikova of Voronezh devoted a small article to the same topic. I. Ia. Levitas of Gor'kii defended a dissertation on the attitude of the Russian intelligentsia toward the events of the second American revolution. In his noted monograph on Lincoln, R. F. Ivanov illumined in great detail Russia's position during the war. Finally, Malkin himself published an article on this subject in the symposium *On the Centennial of the Civil War in the USA.* Certain new archival documents have been made public, the Russian press has been thoroughly studied, and even new problems such as Chernyshevskii's attitude toward the "Provisional Constitution" of John Brown have been taken up.[40] It should be pointed out, nevertheless, that the most detailed and authoritative work on Russia's position during the Civil War remains the old monograph of Malkin. Its level of research and the breadth of coverage of archival and published sources remain unsurpassed, although at present its shortcomings, too, have become much more evident (for example, the book is not well organized, it does not bring out the special attitude of Russian public opinion toward the Civil War, and it contains inaccuracies and gaps).

The second major achievement of Soviet Americanists (here the reference is above all to new works or, at any rate, to books published since the second half of the 1950's) is the study of the Negro problem, the illumination of the role of the Negroes during the Civil War and especially during Reconstruction, and the analysis of the abolitionist movement and the struggle

A. V. Efimov, "Posylka dvukh russkikh eskadr v Severnoi Ameriku" [The Dispatch of Two Russian Squadrons to North America], *Istorik-Marksist* (No. 3, 1936); M. M. Malkin, "K istorii russko-amerikanskikh otnoshenii vo vremia grazhdanskoi voiny v SShA" [On the History of Russian-American Relations during the Civil War in the USA], *Krasnyi Arkhiv,* III (No. 94, 1939).

[39] *Id., Grazhdanskaia voina v SShA i tsarskaia Rossiia* [The Civil War in the USA and Tsarist Russia], ed. E. V. Tarle (Moscow, 1939), 7.

[40] See I. P. Dement'ev, "N. G. Chernyshevskii i konstitutsiia Dzhona Brauna" [N. G. Chernyshevskii and the Constitution of John Brown], *Voprosy istorii* (No. 12, 1959).

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of Negro slaves from the 1830's to the 1850's. At the same time the role of such leaders as Lincoln, Douglass, John Brown, W. L. Garrison, and many others in the second American revolution has been shown in detail.[41]

The works of the founders of Marxism-Leninism decisively influenced the formation of the general conception of the Civil War in Soviet literature. Marx and Engels were contemporaries of and to a certain degree participants in the events that took place in the US; during the war they published about fifty articles devoted to American topics in the New York *Daily Tribune* and the Viennese paper *Die Presse*. The observations of Marx and Engels concerning the existence of two stages in the Civil War—the transition from war in a "constitutional sense" to war in a "revolutionary sense"—the broad sweep of the popular struggle, and so on leave no doubt that the founders of Marxism viewed the war as a revolution. Emphasizing the particular importance of the second stage of the war, Lenin himself wrote about the great, world historical, progressive, and revolutionary significance of the war.

The works of Efimov have played a substantial role in the concrete study of the causes, nature, and meaning of the Civil War. The author made a detailed analysis of the economic and political antecedents, the role of the masses, and, more particularly, the contribution of the Negroes to the defeat of the Confederacy and the eradication of the shameful institution of slavery, and traced the general course and stages of the development of the revolution in ascending and descending line. The author's theoretical conclusions concerning the war as a peculiar bourgeois-democratic revolution and his analysis of the class make-up of the Radical Republicans who expressed the interests of the progressive circles of the industrial bourgeoisie in the northern states had great importance.[42]

[41] See, e.g., R. F. Ivanov, *Bor'ba negrov za Zemliu i svobodu na Iuge SShA (1865–1877)* [The Struggle of the Negroes for Land and Liberty in the American South (1865–1877)] (Moscow, 1958), and *Avraam Linkol'n i grazhdanskaia voina v SShA* [Abraham Lincoln and the Civil War in the USA] (Moscow, 1964); P. B. Umanskii, *Iz istorii bor'by negrov SShA za svobodu (Negry v abolitsionistskom dvizhenii 30–50-kh gg.* XIX *v.)* [From the History of the Struggle of Negroes in the USA for Freedom (Negroes in the Abolitionist Movement of the 1830's–1850's)] (Kazan, 1963); M. N. Zakharova, *Narodnoe dvizhenie v SShA protiv rabstva 1831–1860* [The Popular Movement in the USA against Slavery, 1831–1860] (Moscow, 1965). For greater details, see I. P. Dement'ev and A. M. Ankhov, "Novye sovetskie issledovaniia po istorii bor'by protiv rabstva negrov v SShA" [New Soviet Studies on the History of the Struggle against Negro Slavery in the USA], *Voprosy istorii* (No. 9, 1965), 136–41.

[42] See esp. Efimov, *Ocherki istorii SShA. Ot otkritiia Ameriki do okonchaniia grazhdanskoi voiny; K stoletiiu grazhdanskoi voiny v SShA,* ed. *id.* and Zubok. For a more detailed discussion of the significance of the works of Efimov in the study of the second American revolution, see R. F. Ivanov, "Problemy istorii grazhdanskoi voiny 1861–1865 gg. v trudakh sovetskikh istorikov" [Problems of the History of the Civil War of 1861–1865 in the Works of Soviet Historians], in *Ot Aliaski do Ognennoi zemli* [From Alaska to Tierra del Fuego] (Moscow, 1967), 169–78.

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In Soviet literature it is a generally accepted or, at any rate, the most widespread view that the central question of the Civil War was that of slavery. "In slavery, as in a focus, the interests of the basic classes and groups of the population of the US converged: workers, farmers, slaves, free Negroes, bourgeoisie, and slaveowners. It was precisely the problem of slavery—the main question—that was to be solved in the Civil War."[43]

The work of Kuropiatnik develops, on the other hand, the idea that in the second American revolution the main question was the agrarian one; slavery was a subordinate problem.[44] The struggle between the North and the South, in the author's opinion, meant nothing but "the struggle of two kinds, or types, of capitalism—capitalism of type A against capitalism of type B—or a struggle of the American type of bourgeois development against variants of the Prussian type."[45] The idea that the problem of two possible roads in the development of capitalism was being resolved in the Civil War is interesting and fruitful, although it is difficult to agree with the complete identification of plantation slavery with capitalism (even if one is to call it "Prussian" or "capitalism of type B"). (We may recall that Marx placed American slavery in the capitalist mode of production "only formally, since the slavery of the Negroes precludes free hired labor.") Even if one fully accepts the author's point of view, however, he cannot deny that in the concrete circumstances in the southern states the agrarian question emerged foremost as a question of the liquidation of slavery. It is characteristic that the fighting slogan of the war, as the author himself notes, was above all "Down with slavery"; only later, after emancipation, the slogan "Forty acres and a mule" gained popularity. There is no doubt that both "main questions" were closely linked, and thus it is not coincidental that Kuropiatnik emphasizes that the radical solution of the agrarian question meant at the same time the destruction of slaveownership "inasmuch as the liquidation of big landownership in the US at that time was also the dismantling of slave-owning estates and liquidation of the slaveowners as a class."[46]

For a fundamental and comprehensive solution of theoretical and concrete problems of the Civil War and Reconstruction a basic work like the well-known symposia on the history of the Revolutions of 1848–1849, the French Revolution of 1789–1794, and the English revolution of the seventeenth century has long been needed. This does not, of course, preclude the

[43] Ivanov, *Avraam Linkol'n i grazhdanskaia voina v SShA*, 37.
[44] See G. P. Kuropiatnik, *Vtoraia amerikanskaia revoliutsiia* [The Second American Revolution] (Moscow, 1961), 4.
[45] *Ibid.*, 6, 225, *et passim*.
[46] *Ibid.*, 4–5.

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simultaneous pursuit of monographic investigations of individual and general problems of the Civil War and, in particular, of the agrarian question.

This is valid to an even greater degree for the War of Independence and the formation of the United States. At present an altogether paradoxical situation has arisen in Soviet historiography: there is a whole school concerned with the Civil War and not a single serious monograph devoted to the American Revolution. Works relating to the revolutionary period and the formation of the US can easily be counted on one's fingers: the popular outline of A. A. Fursenko (1960), the dissertations of B. D. Syrkin on the Constitution of 1787 (1949) and of L. N. Goncharov on the Bill of Rights (1963), and several works on individual leaders in the War of Independence such as Washington, Franklin, Jefferson, and Paine. I have had occasion since the end of the 1950's to treat in detail the question of Russia's position during the Revolution, first in a special article and then in the appropriate section of the monograph on Russian-American relations.[47] This is, however, only a special and secondary question if one considers the whole problem of the Revolution at the end of the eighteenth century. The coming bicentennial of the War of Independence will serve, we should hope, as an additional stimulus for Soviet historians to begin the systematic study of the Revolution; the first steps in this direction are already being taken. P. B. Umanskii of Kazan has begun to study the historiography of the war, and Fursenko continues to work on general problems of the Revolution. We hope that historians in Moscow will join in studying this period of American history.

Foreign policy is the best-developed field of US history among Soviet historians. Earlier I spoke of a relatively small number of works; at most what was involved was, taking as an example the Civil War and Reconstruction, a few dozen studies. In the field of foreign policy the total number of books and articles approaches 200 (since 1917 some 40 books and about 150 articles have been published on American foreign policy and diplomacy in the modern period). The level of professionalism and the scholarly importance of these works vary greatly. Few have passed the test of time and retained their scholarly value even though they were devoted, it would seem, to topical subjects. In a number of cases their documentary foundation was inadequate. There are, finally, many problems that were not illu-

[47] N. N. Bolkhovitinov, "Russkaia diplomatiia i voina SShA za nezavisimost' 1775–1783 gg." [Russian Diplomacy and the American War of Independence of 1775–1783], *Novaia i Noveishaia Istoriia* (No. 1, 1964), and "Rossiia i revoliutsionnaia voina za nezavisimost' v Severnoi Amerike v XVIII veke" [Russia and the Revolutionary War of Independence in North America in the 18th Century], in *id., Stanovlenie russko-amerikanskikh otnoshenii 1775–1815* [The Establishment of Russian-American Relations, 1775–1815] (Moscow, 1966), Pt. 1.

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mined at all or were very superficially touched. As a whole, the contribution of Soviet historians to the study of American foreign policy and diplomacy is, however, quite substantial, and it cannot be ignored even by our ideological adversaries.

Soviet investigators are especially concerned with the problem of the expansion of the United States in the Western Hemisphere, the Pacific Basin, and the Far East. Propagandizing the theory of the "exceptional nature" of American capitalism, Western and, above all, American historiography has suggested that, in contrast to England, France, tsarist Russia, and other countries that fought aggressive wars and seized others' territories, the US only "acquired" and "bought" them and rendered "assistance" to other nations. On the basis of enormous amounts of documentary material Soviet historians unmask these assertions, demonstrate the annexationist endeavors of the ruling circles of the US, and investigate the internal causes of the expansionist policy determined at first by the interests of the planters and slaveowners of the South and the upper bourgeoisie of the North and later, in the Age of Imperialism, by the interests above all of monopoly capital. It should be noted that the progressive forces in the United States have striven unswervingly, in line with the old liberating, democratic, and revolutionary traditions of the American people, toward peace and have resisted the policy of war, aggression, and expansion.

The largest synthesizing work on the history of American foreign policy is the book by N. N. Inozemtsev. Although his work deals essentially with the period after 1917, the author provides a brief but expert outline of the evolution of American foreign policy in the nineteenth and the beginning of the twentieth centuries, to a certain extent summing up the results of preceding Soviet investigations.[48]

The study of American foreign policy and diplomacy has, on the whole, proceeded unevenly. Attention has been devoted mainly to American policy in the Far East and Latin America. American relations with European countries in particular have, on the other hand, been almost neglected.[49] There are also many gaps in the study of the earlier stages of American foreign policy (at the end of the eighteenth and the first half of the nineteenth centuries).

There is nothing astounding in the broad scale of the study of American policy in the Far East and in the entire basin of the Pacific Ocean. In 1935

[48] See N. N. Inozemtsev, *Vneshniaia politika SShA v epokhu imperializma* [The Foreign Policy of the USA in the Age of Imperialism] (Moscow, 1960), 9–127.

[49] This is true if one does not count the book of Z. M. Gershov, *"Neitralitet" SShA v gody pervoi mirovoi voiny* [The "Neutrality" of the USA in the Years of World War I] (Moscow, 1962).

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A. Ia. Kantorovich published a highly valuable investigation that, on the basis of documentary material, exploded the myth of the peace-loving policy of the US in regard to China.[50] The books of V. Ia. Avarin on Japanese-American and Anglo-American contradictions in the struggle for the Pacific Ocean are well known; they also devote some attention to problems of the nineteenth and the beginning of the twentieth centuries.[51] A. A. Guber, B. A. Romanov, and A. L. Narochnitskii have produced the basic works on international relations concerning the Pacific and, more particularly, American policy in Asia.[52] A brilliant literary form and mastery of the use of documentary sources in many languages assured the popularity of Guber's book in wide reading circles. Narochnitskii, using a huge mass of documentary and, above all, archival materials, demonstrated that, along with the old colonial countries (England and France), the United States also pursued an active colonial policy in the Far East in the nineteenth century. S. S. Grigortsevich of Tomsk produced something of a continuation of Narochnitskii's monograph; it is devoted to events at the beginning of the twentieth century.[53] There are, in general, more than enough followers and continuators of the authors cited, and at present we can be proud of the achievements of a considerable group of investigators who have made contributions to the study of American policy in the Pacific.

Fursenko, S. B. Gorelik, and R. M. Brodskii of Lvov, among others, have treated comprehensively the struggle of the United States for the partition of China and the proclamation of the "Open Door."[54] A. Dobrov has made a profound study of the position of the US during the Russo-Japanese War.[55] In a similar way, by drawing upon primary sources, P. P. Sevost'ianov has

[50] A. Ia. Kantorovich, *Amerika v bor'be za Kitai* [America in the Struggle for China] (Moscow, 1935).

[51] V. Ia. Avarin, *Bor'ba za Tikhii okean* [The Struggle for the Pacific] (2 vols., Moscow, 1947–52).

[52] A. A. Guber, *Filipinskaia respublika 1898 g. i amerikanskii imperializm* [The Philippine Republic of 1898 and American Imperialism] (Moscow, 1948; 2d ed., 1961); *Mezhdunarodnye otnosheniia na Dal'nem Vostoke* [International Relations in the Far East], ed. E. M. Zhukov (Moscow, 1951; 2d ed., 1956); A. L. Narochnitskii, *Kolonial'naia politika kapitalisticheskikh derzhav na Dal'nem Vostoke. 1860–1895* [The Colonial Policy of the Capitalist States in the Far East. 1860–1895] (Moscow, 1956); B. A. Romanov, *Ocherki diplomaticheskoi istorii russko-iaponskoi voiny. 1895–1907* [Outlines of the Diplomatic History of the Russo-Japanese War. 1895–1907] (Moscow, 1947; 2d ed., 1955).

[53] S. S. Grigortsevich, *Dal'nevostochnaia politika imperialisticheskikh derzhav v 1906–1917 gg.* [The Far Eastern Policy of the Imperialist States in 1906–1917] (Tomsk, 1965).

[54] A. A. Fursenko, *Bor'ba za razdel Kitaia i amerikanskaia doktrina "otkrytykh dverei"* [The Struggle for the Partition of China and the American "Open Door" Doctrine] (Moscow, 1956); S. B. Gorelik, *Politika SShA v Man'churii v 1898–1903 gg. i doktrina "otkrytykh dverei"* [The Policy of the USA in Manchuria in 1898–1903 and the "Open Door" Doctrine] (Moscow, 1960); R. M. Brodskii, *Amerikanskaia ekspanziia v Severo-Vostochnom Kitae. 1898–1905* [The American Expansion in North-East China. 1898–1905] (Lvov, 1965).

[55] A Dobrov, *Dal'nevostochnaia politika SShA v period russko-iaponskoi voiny* [The Far Eastern Policy of the USA in the Years of the Russo-Japanese War] (Moscow, 1952).

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analyzed the expansionist policy of the US in China and Korea in 1905–1911.[56] Kuropiatnik, D. D. Tumarkin, and K. V. Malakhovskii have treated the history of the prolonged struggle of the US to seize a series of islands in the Pacific.[57] D. V. Petrov has devoted a special work to the initial stage of the American penetration of Japan.[58]

Even this incomplete listing of the printed literature provides some idea of the scale of the work of Soviet historians in the study of American policy in the Pacific, especially in the Far East.

The history of American relations with Latin America has also received serious attention in recent years; most thoroughly studied is the Age of Imperialism. Since the publication of Zubok's well-known monograph on American policy in the countries of the entire Caribbean Basin,[59] there have appeared a considerable number of books and articles on various concrete questions and individual countries by, among others, M. S. Al'perovich, E. V. Ananova, L. S. Vladimirov, S. A. Gonionskii, A. M. Zorina, M. A. Okuneva, L. Iu. Slezkin, and K. S. Shustov of Alma Ata. Special books have been devoted to the Spanish-American War and the imperialist policy of the US toward Cuba,[60] the seizure of the Panama Canal,[61] and the unmasking of the interventionist nature of American policy during the Mexican Revolution of 1910–1917.[62]

The Latin American policy of the US during the nineteenth century has been studied in much less detail. In addition to my monograph on the Monroe Doctrine and articles on the position of the US during the Latin American war of independence,[63] one may cite the book of N. V. Potokova of

[56] P. P. Sevost'ianov, *Ekspansionistskaia politika SShA na Dal'nem Vostoke (v Kitae i Koree v 1905–1911 gg.)* [The Expansionist Policy of the USA in the Far East (In China and Korea in 1905–1911)] (Moscow, 1958).

[57] G. P. Kuropiatnik, *Zakhvat Gavaiskikh ostrovov* [The Seizure of the Hawaiian Islands] (Moscow, 1958); D. D. Tumarkin, *Vtorzhenie kolonizatorov v "krai vechnoi vesny"* [The Intrusion of the Colonizers in the "Land of the Eternal Spring"] (Moscow, 1962); K. V. Malakhovskii, *Bor'ba imperialisticheskikh derzhav za Tikhookeanskie ostrova* [The Struggle of the Imperialist States for the Pacific Islands] (Moscow, 1966).

[58] D. V. Petrov, *Kolonial'naia ekspanziia Soedinennykh Shtatov Ameriki v Iaponii v seredine* XIX *veka* [The Colonial Expansion of the United States of America in Japan in the Mid-19th Century] (Moscow, 1955).

[59] L. I. Zubok, *Imperialisticheskaia politika SShA v stranakh Karibskogo basseina. 1900–1939* [The Imperialist Policy of the USA in the Countries of the Caribbean Basin. 1900–1939] (Moscow, 1948).

[60] L. Iu. Slezkin, *Ispano-amerikanskaia voina* [The Spanish-American War] (Moscow, 1956); L. S. Vladimirov, *Diplomatiia SShA v period amerikano-ispanskoi voiny 1898 g.* [The Diplomacy of the USA during the American-Spanish War of 1898] (Moscow, 1957); K. S. Shustov, *Zhemchuzhina Antill i ianki (Kuba i imperialisty SShA). 1899–1952* [The Pearl of the Antilles and the Yankees (Cuba and US Imperialists). 1899–1952] (Alma-Ata, 1967).

[61] S. A. Gonionskii, *Istoriia panamskoi "revoliutsii"* [The History of the Panamanian "Revolution"] (Moscow, 1958).

[62] M. S. Al'perovich and B. T. Rudenko, *Meksikanskaia revoliutsiia 1910–1917 gg. i politika SShA* [The Mexican Revolution of 1910–1917 and the Policy of the USA] (Moscow, 1958).

[63] See, e.g., Bolkhovitinov, *Doktrina Monro*, and "K voprosu o positsii SShA v voine Latinskoi Ameriki za nezavisimost'" [On the Question of the Position of the USA in the Latin

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Groznyi on the American war against Mexico in 1846–1848[64] and the articles by Ananova, M. R. Matveeva of Kalinin, G. I. Ivanov of Ivanovo, Slezkin, and V. Smolenskii on several questions of American relations with Latin America in the nineteenth century.[65]

Soviet historiography has not had deep traditions in the study of the Latin American policy of the US (as in the study of the history of Latin America as a whole); the basic work at present is the result of only the last ten to fifteen years. This, of course, influences the professional level of individual works. The required perseverance has not always been demonstrated in searching for rare documentary sources, including materials held by Soviet archives.

The problem most deeply and comprehensively illumined by Soviet investigators is American expansion in the Western Hemisphere in the first half of the nineteenth century and in the Age of Imperialism. At the same time, almost no serious attention has been given to other aspects of American policy. This reproach also should be directed against my work on the Monroe Doctrine. The book studied the influence of the tendencies toward expansion and nationalism on the origin and nature of the doctrine. The book's conclusions have received a certain recognition and have even played some part in advancing the historiography of the subject (more particularly, the study of the Russian position during the Latin American war of independence).[66]

Persuasive evidence, however, is only one side of the matter, even if it is in this case the main aspect that was misinterpreted earlier; it leads unintentionally to leaving the other aspects of the problem in the shadows and even to covering them over. A real danger of intervention by the Holy Alliance in Latin America in the 1820's did not exist; all informed investigators now agree with this. This does not mean, of course, that the leaders of the Holy

American War of Independence], in *Voina za nezavisimost' v Latinskoi Amerike (1810–1826)* [The War of Independence in Latin America (1810–1826)] (Moscow, 1964).

[64] N. V. Potokova, *Agressiia SShA protiv Meksiki. 1846–1848* [The Aggression of the USA against Mexico. 1846–1848] (Moscow, 1962).

[65] See, e.g., E. V. Ananova, "Amerikanskie plany anneksii Dominikanskoi respubliki v XIX v" [The American Plans for Annexation of the Dominican Republic in the 19th Century], in *Ot Aliaski do Ognennoi zemli;* M. R. Matveeva, "Pervye popytki ustanovleniia otnoshenii SShA s Paragvaem (40-e gody XIX veka)" [The First Attempts toward Establishment of Relations between the USA and Paraguay (1840's)], *Uchenye Zapiski* (Kalininskii pedagogicheskii institut), XXXVIII (1964); G. I. Ivanov, "Bor'ba meksikanskogo naroda protiv amerikanskoi agressii" [The Struggle of the Mexican People against American Aggression], *Uchenye Zapiski* (Ivanovskii pedagogicheskii institut), XI (1957); L. Iu Slezkin, "Grazhdanskaia voina v SShA i ikh iuzhnye sosedi" [The Civil War in the USA and Its Southern Neighbors], in *K stoletiiu grazhdanskoi voiny v SShA,* ed. Efimov and Zubok; V. Smolenskii, "SShA i Tikhookeanskaia voina 1879–1884 gg." [The USA and the Pacific War, 1879–1884], *Novaia i Noveishaia Istoriia* (No. 3, 1967).

[66] See L. Iu. Slezkin, *Rossiia i voina za nezavisimost' v Ispanskoi Amerike* [Russia and the War of Independence in Spanish America] (Moscow, 1964); M. Kossok, *Im Schatten der Heiligen Allianz* (Berlin, 1964).

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Alliance were not extreme reactionaries or that they did not want to smother the revolution in any region, including Latin America. They wanted to do so, but were unable to, and for precisely this reason they did not make any real plans. The main elements in the Monroe Doctrine were, in my opinion, the tendencies toward expansion and nationalism; precisely these tendencies determined in practice its use by the ruling circles of the US. All this does not mean that in concrete circumstances (for example, in the period of French intervention in Mexico in the 1860's) this doctrine could not be used for defensive purposes. The bourgeois principles, progressive for their time, even though inconsistent, that the message of 1823 embodied, found an echo in the popular masses of the US who sincerely sympathized with the struggle of Spanish America for independence. The ruling circles in Washington used these same principles in pursuing their expansionist and egotistic objectives. The complexity and internal contradiction of historical events always demand from the historian maximum vigilance and objectivity. These elements were, generally speaking, reflected in the text, but they turned out to be, as it were, in the background. No serious documentary sources were provided to back them up, as was done, let us say, in the case of evidencing the absence of any real danger of intervention by the Holy Alliance, and the text of the book appeared, especially at a cursory glance, somewhat one-sided.

The last question to be discussed relates to a subject close to my scholarly interests: the history of relations between Russia and the US. My work on the establishment of Russian-American relations in the eighteenth and the beginning of the nineteenth centuries solved to a certain degree only a segment of the problem (a very small one at that), and the study of the development of the entire nexus of diplomatic, commercial, sociopolitical, cultural, and scientific ties between Russia and the US is only beginning.

As has been noted above, Soviet historians have scored significant successes in the study of Russian-American relations during the Civil War (Malkin's monograph and other works). The period from the end of the nineteenth century to 1917 has been more or less satisfactorily investigated (in the area of economic ties, at any rate). Here, above all, deserve to be mentioned the superb works of Fursenko—his monograph on the "oil trusts" and his article on Russian-American financial ties at the turn of the century, both based on original archival materials[67]—the book of V. V. Lebedev examining the economic relations between Russia and the US in the Age of Imperial-

[67] A. A. Fursenko, *Neftianye tresty i mirovaia politika. 80-e gody–1918 g.* [The Oil Trusts and World Politics. 1880's–1918] (Moscow, 1965), and "Iz istorii russko-amerikanskikh otnoshenii na rubezhe XIX–XX vv." [From the History of Russian-American Relations at the Turn of the Century], *Trudy Leningradskogo otdeleniia Instituta Istorii AN SSSR* (No. 1, 1959).

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ism,[68] the dissertations of V. Kiselev, A. A. Lukin of Tomsk, and especially I. M. Rabinovich of Leningrad concerning the penetration of American capital in Russia prior to 1917,[69] and the articles of Z. M. Gefter and R. Sh. Ganelin of Leningrad.[70]

The book by Professor S. B. Okun' of Leningrad on the history of the Russian American Company, grounded to a considerable degree in original Russian archival sources, gained recognition in Soviet and foreign historiography.[71] The dissertations of N. V. Sverdlin of Khabarovsk and T. M. Batueva treat American expansion into Russian possessions in the Northern Pacific in the second half of the nineteenth century.[72]

It is regrettable that the "blank spots" in the study of the history of relations between Russia and the US are still more numerous than the problems that have been studied. The 1820's, 1830's, 1840's, 1850's, 1870's, and 1880's still remain an almost virgin field for the investigator. This is also true of the history of sociopolitical, cultural, and scientific ties between the two countries in the nineteenth and the beginning of the twentieth centuries and of economic relations during almost the entire nineteenth century.

Many other problems already dealt with in the literature also need more profound study. Thus, for example, the body of documentary materials on the activity of the Russian American Company has been substantially widened at the present time. In the 1950's a special collection on the history of this company and of Russian possessions in North America was set up in the Archives of the Foreign Policy of Russia.[73] Microfilms have been ob-

[68] V. V. Lebedev, *Russko-amerikanskie ekonomicheskie otnosheniia (1900–1917)* [Russian-American Economic Relations (1900–1917)] (Moscow, 1964).

[69] V. Kiselev, *Proniknovenie amerikanskogo imperializma v dorevoliutsionnuiu Sibir'* [The Penetration of American Imperialism in Prerevolutionary Siberia] (Moscow, 1955); A. A. Lukin, *Proniknovenie amerikanskogo i angliiskogo Kapitala v Zapadnuiu Sibir' nakanune i v gody pervoi mirovoi voiny* [The Penetration of American and English Capital in Western Siberia on the Eve and during World War I] (Tomsk, 1952); I. M. Rabinovich, *Proniknovenie amerikanskogo kapitala v ekonomiku dorevoliutsionnoi Rossii (1900–1917)* [The Penetration of American Capital in the Economy of Prerevolutionary Russia (1900–1917)] (Leningrad, 1957).

[70] See, e.g., Z. M. Gefter, "Iz istorii proniknoveniia amerikanskogo kapitala v tsarskuiu Rossiiu do pervoi mirovoi voiny" [From the History of the Penetration of American Capital in Tsarist Russia prior to World War I], *Istoricheski Zapiski* (No. 35, 1950); R. Sh. Ganelin, "Rinansovo-ekonomicheskie otnosheniia Rossii i SShA posle nachala mirovoi voiny (avgust 1914–noiabr' 1915)" [Financial and Economic Relations of Russia and the USA after the Beginning of World War I (August 1914–November 1915)], in *Iz istorii imperializma v Rossii* [From the History of Imperialism in Russia] (Moscow, 1959).

[71] S. B. Okun', *Rossiisko-amerikanskaia kompaniia* [The Russian-American Company] (Moscow, 1939).

[72] N. V. Sverdlin, *Amerikanskaia ekspansiia na severo-vostoke Rossii v kontse* XIX *v.* [The American Expansion in the Russian North-East at the End of the 19th Century] (Moscow, 1951); T. M. Batueva, *Amerikanskaia ekspansiia v russkikh vladeniiakh na severe Tikhogo okeana v seredine* XIX *v. i pokupka Aliaski SShA v 1867 g.* [The American Expansion in the Russian Possessions in the Northern Pacific in the Middle of the 19th Century and the Purchase of Alaska by the USA in 1867] (Moscow, 1953).

[73] See "Sobranie dokumental'nykh materialov po istorii Rossiisko-amerikanskoi kompanii i russkikh vladenii v Severnoi amerike" [Collection of Documentary Materials on the History

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tained of the rich materials concerning the activity of the Russian American Company that were given to the United States after the sale of Alaska.[74] This must, of course, create the need to conduct a new investigation of the history of Russian America.

It is hardly necessary to prove the need of the investigator to use archival materials of both sides (Russian and American). The complete sets of reports of American envoys from St. Petersburg, instructions of the State Department, and correspondence of the Russian legation in Washington with the American government,[75] received as a result of the cooperation of Soviet and American libraries and archives, now allow the hope that in the future the level of the study of Russian-American relations will be considerably raised.

A few final words on historiography are in order. Soviet journals publish a large number of surveys and reviews of the works of American historians, but a systematic study of the development of historical scholarship in the US is lacking; the only monograph on this development is the detailed investigation by I. P. Dement'ev that examines the general trends in the evolution of the historiography of the Civil War in close relation to the class and political struggles in the US.[76] Important problems related to the development of American historiography are dealt with in the well-known books of I. S. Kon of Leningrad and E. B. Cherniak[77] and in the special articles by Efimov, Bolkhovitinov, Dement'ev, V. L. Mal'kov, and Askol'dova devoted to such leading American historians as Frederick Jackson Turner, Charles A. Beard, and John R. Commons.

One must also draw special attention to the great value of detailed critical surveys of the literature that have become an integral part of dissertations and of the majority of monographic works. The historiographic introduction (sometimes the survey is provided in a special appendix) acquires particular value inasmuch as it is written on the basis of the author's personal investigation which, as a rule, guarantees a competent evaluation of the preceding literature.

of the Russian-American Company and the Russian Possessions in North America], Arkhiv vneshnei politiki Rossii, fol. 888.

[74] Records of the Russian-American Company, 1802–1867, US National Archives.

[75] Dispatches from the United States Ministers to Russia, 1808–1906, *ibid.*; Diplomatic Instructions of the Department of State, 1801–1906, *ibid.*; Notes from the Russian Legation to the United States Department of State, 1809–1909, *ibid.*

[76] I. P. Dement'ev, *Amerikanskaia istoriografiia grazhdanskoi voiny v SShA (1861–1865)* [The American Historiography of the Civil War in the USA (1861–1865)] (Moscow, 1963).

[77] I. S. Kon, *Filosofskii idealizm i krizis burzhuaznoi istoricheskoi mysli* [Philosophic Idealism and the Crisis of Bourgeois Historical Thought] (Moscow, 1959); E. B. Cherniak, *Burzhuaznaia istoriografiia rabochego dvizheniia (kriticheskii ocherk)* [The Bourgeois Historiography of the Labor Movement (A Critical Survey)] (Moscow, 1960), and *Istoriografiia protiv istorii* [Historiography against History] (Moscow, 1962).

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To sum up, one should first of all note the numerical and, what is very important, qualitative growth of Soviet works on the history of the US, published mostly during the last ten to fifteen years. Prior to the October Revolution the original Russian works on American history could be counted on one's fingers, and on close examination one finds that the majority of them turned out to be compilations.

At present, scores and, if one also takes into account contemporary history, hundreds of special books and articles are published on the history of the US each year. It is understandable that their professional level and scholarly value vary. On the whole the progress of Soviet American studies is, however, absolutely plain. We understand, of course, that little has yet been done and that much more remains to be solved, as, for example, the problem of the so-called "initial accumulation of capital" in the seventeenth and eighteenth centuries, the basic stages and special features of the Industrial Revolution, the development of capitalism in agriculture in the nineteenth century, the formation and development of the American nation, the history of utopian socialism, and the history of religion.

The present work of investigation has only begun. That it has begun, however, there should be no doubt.

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★ ★ ★ ★ *Reviews of Books* ★ ★ ★ ★

General

THE TRANSFER OF IDEAS: HISTORICAL ESSAYS. Edited by *C. D. W. Goodwin* and *I. B. Holley, Jr.* (Durham, N. C.: South Atlantic Quarterly. 1968. Pp. ix, 179. $6.00.)

THE EMERGENCE OF THE EASTERN WORLD: SEVEN ESSAYS ON POLITICAL IDEAS. By *G. L. Seidler.* With a foreword by *Sir Isaiah Berlin.* (New York: Pergamon Press. 1968. Pp. xi, 260. $11.00.)

HANDLING these two books in a single review must be justified by their intent rather than by their content. Both books attempt to broaden the perspective from which professional historians conventionally work, which is a commendable task given the essentially conservative nature of professional historians where epistemology and methodology are concerned. The essays collected by C. D. W. Goodwin and I. B. Holley, Jr., are intended to direct the historian's attention to the new disciplines connected with communication theory, as a means of illuminating the problem of how ideas are transferred from one cultural context to another, the barriers they meet in the process of migration, and the transformations they undergo as a result. G. L. Seidler, rector of Marie Curie-Skłodowska University in Lublin, Poland, tries to dissolve what he views as a distinctive "European centrism" in contemporary Western historiography through the adumbration of what must be called (in his account) a "non-Western tradition" of political thought. Both works seek to contribute—through interdisciplinary exchange on the one side and intercultural dialogue on the other—to the historiography of the history of ideas.

It is unfortunate, though hardly surprising, given the difficulty of the tasks undertaken, that neither work achieves its original purpose. Although both works contain much that is "informative and interesting," neither contributes substantially to the general methodological and interpretive problems that it sought to illuminate and that were intended to justify the publication of the disparate essays under a single cover.

In the case of the Goodwin and Holley volume, this unfortunate result appears to stem from an excess of virtue rather than of vice. The essays included in their work are uniformly marked by what would ordinarily be regarded as commendable scholarly caution. They range from microstudies such as those by Echols, on the introduction of shell architecture into the United States, and by Staude, on attempts to transfer the Workers' Councils from Russia to Germany in 1918; through mesostudies such as R. S. Smith's account of the transfer of English economic ideas to Spain and C. W. D. Goodwin's study of economic ideas in the development of Jamaica; to macrostudies like that of Hamilton on the transmission of English law to the American frontier and that of Hillerbrand on the spread of the Protestant Reformation in the sixteenth century. Each is

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professionally competent: detailed, well documented, careful in delineating problems and deriving conclusions. But they contribute to the understanding of the general problem of how ideas are communicated "from nation to nation and from culture to culture" only in the most general way because each of the instances is too specifically analyzed to permit extrapolation to the explanatory model desired by the editors. The difficulty seems to lie in the editors' hope that some general theoretical implications could be derived from close empirical studies uninformed by prior hypotheses. But, if general theory is the aim, then some kind of hypothesis has to be the beginning. As Bacon said, error is more productive than confusion, at least when science is the issue.

In short, the modesty and caution of the editors are fatal to their central purpose. If one is going to open the *methodological* can of worms, it should then be investigated with speculative daring. The sparse, concluding note on how communication theory might be used to illuminate the process by which ideas are spread is unsatisfying; it is more likely to lock historians into their anti-theoretical biases than to entice them away. Yet, to the editors' credit, they at least recognize that there is a problem, and they are unwilling to follow the well-trodden, aimless paths mapped out in the conventional history of ideas. Each essay in the collection will interest specialists in its own field or area, but the general historian or the historian of ideas concerned with theoretical matters is not likely to gain much from the collection as a whole.

Seidler's book is another kettle of fish, or can of worms. Its theme is "the emergence of the Eastern world," and it deals with seven traditions of political thought: ancient Near Eastern, Muslim, Byzantine, Mongol, Central European conciliar, Muscovite, and Jagiellonian. The account, as I read it, suffers from three kinds of weaknesses: theoretical, scholarly, and pedagogical. First, if his purpose is to characterize a distinctively "Eastern" political tradition and to set it over against a "Western" one, it is difficult to find a basis for this characterization, or even to construct it. He appears to group all of these diverse traditions together under a single rubric only because they are not Western. What makes them "non-Western" is never clearly stated, and I found it impossible to deduce, from his account, their common, or shared, family traits. On the scholarly level, Seidler seems totally unaware of post-World War II "Western" contributions to our understanding of the obscure ancient Near Eastern, Islamic, and Biblical Hebrew traditions. This undercuts his pedagogical purpose of moving Western scholars out of their "European centrism," because it completely misconstrues current "Western" scholarship on those matters with which Seidler is concerned. This is certainly true in his accounts of Mesopotamian, Egyptian, Biblical, Islamic, Byzantine, and conciliar political thought; I am no judge of Mongol, Muscovite, and Jagiellonian matters. In any event, intercultural dialogue works both ways, and, as they say in communication theory, a signal from a source comes through as noise unless sender and receiver are on the same wave length.

In the interest of intercultural dialogue, I want to stress the fact that my objections to Seidler's study have nothing to do with the Marxist cast of its general structure. The general interpretive apparatus of the work consists largely of a mild brand of dialectical materialism, phrased in the economic deterministic jargon of what Lenin called "vulgar materialism." I think that dialectical materialism is a possible perspective on historical problems with at least as much

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claim to a hearing as its bourgeois counterparts. But there is a real question regarding its possible application to the problems that concern Seidler here. In *Geschichte und Klassenbewusstsein* (1921), the Marxist theorist Georg Lukács argued that dialectical materialism was not an effective analytical tool when turned upon the literature of prebourgeois societies and their cultural products. Before the bourgeoisie succeeded in demythifying the world, he argued, neither thought nor action was governed by immediately experienced class relationships; these relationships were mediated in distinctive ways that required other—psychological and cultural—modes of analysis for their comprehension. The same case might be made for the attempt to provide a class interpretation of ancient and medieval political thought, whether of the Eastern or the Western traditions. Even if we grant that all history involves class struggle, we still might not be able to document that fact from the evidence available to us for such specific fields of history as the ancient Near East. In short, our inability to write a convincing class-based history of ancient or medieval political thought may arise from the nature of the evidence rather than a fault in Marxist theory. If that were so, the historian's tact would be measured by his ability to decide where a dialectical materialistic approach to the materials was applicable or appropriate in an epistemological sense, and where it was not. In some periods of study, in short, the categories of interpretation may be dictated by the nature of the surviving documents rather than by the existential situation of the past groups and cultures being studied. Wilamowitz-Möllendorf once said that we could not write an adequate biography of any ancient Greek or Roman; it may well be that we cannot have a dialectical materialistic history of ancient and medieval thought either, and for the same reasons: older cultures did not produce or collect the kind of data that would permit us to fill out the categories required by good Marxist theory.

Perhaps Hegel is, after all, our best guide in such matters. In following him we should not be constrained to produce works such as Seidler's, which oscillates between cryptic generalizations that obscure more than they illuminate on the one side and simple chronicle on the other. And perhaps we would not even have to write such sentences as the following, which is part of his analysis of the decline of dynastic fortunes in the face of rising national sentiment: "The national ties were more durable because they were significant for all social classes. They were in keeping with the logic of history, while the other [dynastic] bonds could not last because they were of a more temporary character."

University of California, Los Angeles HAYDEN V. WHITE

PROBLÈMES DE STRATIFICATION SOCIALE: ACTES DU COLLOQUE INTERNATIONAL (1966). Published by *Roland Mousnier*. [Publications de la Faculté des Lettres et Sciences humaines de Paris—Sorbonne. Series "Recherches," Number 43. Travaux du Centre de Recherches sur la Civilisation de l'Europe Moderne, Number 5.] (Paris: Presses Universitaires de France. 1968. Pp. 283. 30 fr.)

IN 1965 Professor Roland Mousnier set out a schema for the analysis of social stratification in history, by which all societies should be allocated to one of three

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very carefully defined groupings. The first, a society of castes, is stratified according to degrees of hereditary religious purity (only India fits this type); the second, of orders and estates, is stratified according to degree of status, honor, and dignity; the third, of classes, is stratified according to roles played in the productive process and the income earned therefrom. In 1966 he organized an international colloquium to discuss the utility of such a tripartite typology, and this book is a collection of the papers read there, together with a précis of the discussions that followed each session. There were contributions on Germany (K. Bosl, W. Conze, M. Stuke, and F. L. Carsten), Sweden (M. Palme), the Middle Ages (L. Génicot), the United Provinces (I. Schoffer and M. I. J. Brugmans), Rome (M. Giuntella), England (G. E. Aylmer and D. Marshall), and France (the late Professor Cobban's largely irrelevant onslaught on M. Godechot's review of his book on *The Social Interpretation of the French Revolution*).

The papers, ranging from the detailed case study to the synoptic survey, are of varying quality. Few if any of the contributors seem very happy with the proposed typology, while many show a somewhat irritable distaste for any such borrowings from the social sciences. Indeed, what is depressing about this volume is the degree of ignorance concerning the work on social stratification carried out by sociologists (mostly American) displayed by so many highly respected and distinguished European historians. The objections of those who were in principle sympathetic to the endeavor can be summarized as follows: There was an almost universal dislike of Mousnier's definition of caste, mainly because it is wholly irrelevant to the European scene; many would like to apply the word to any more or less closed hereditary social group. Second, many contributors pointed to the limited explanatory value of the distinction between orders and classes. Between the twelfth and the eighteenth century, European societies were stratified partly according to hereditary lineage, partly according to rank acquired by blood, state service, or purchase, and partly according to wealth. There was considerable, but by no means perfect, congruence between these three factors, and, as Professor Labrousse has pertinently observed, the differences in the modes of stratification are more interesting and in most ways more important than the similarities. Even in the admittedly class-structured societies of nineteenth-century England and Prussia, moreover, the hereditary aristocracy still played a very important political and economic role and imposed its ancient system of values on large sectors of society. Finally, it was observed that a wide gap often developed between constitution and law on the one hand and social reality on the other; the former, as in eighteenth-century France, clung to orders and estates long after the latter had become increasingly geared to competitive economic production and the values associated with it.

During this three-day colloquium a serious failure of communication evidently developed between Mousnier and many of his colleagues. Mousnier failed in his efforts to explain that he was talking about Weberian ideal types that may never have existed in reality but whose identification serves to illuminate some of the most basic movements in history. His distinction between a society stratified primarily, but not exclusively, by orders and a society stratified primarily, but not exclusively, by classes is both important and true, and this is unaffected by Labrousse's perfectly valid observation that "L'Histoire, notamment l'histoire

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sociale, aime l'impur." More serious is Professor Duby's equally valid observation that any society is stratified along a whole series of lines that are in theory mutually incompatible. But Western European societies *did* move, irregularly and independently, from orders to classes somewhere between the early sixteenth and the late nineteenth century. Unfortunately this does not tell us very much about any given European society, how it differed in its structure and internal organization from other societies, or the stages of its development. As these essays make abundantly clear, there were enormous differences from society to society in the relative weight in terms of prestige and legal privilege attached to birth, state service, education, titular rank, income, and source of income. This volume is a useful contribution to the study of social history because it draws attention to these differences. As Mousnier remarked in his closing speech, the early modern period, with which he and most of the authors are primarily concerned, is a fluid, transitional era, and he called for a large-scale cooperative inquiry in order to work out a typology of societies of orders that will satisfy both the historian in his search for the empirical reality and the social scientist in his demand for a meaningful system of classification. This is the task of the 1970's, building on the foundations laid by this volume.

Princeton University LAWRENCE STONE

THE STUDY OF URBAN HISTORY: THE PROCEEDINGS OF AN INTERNATIONAL ROUND-TABLE CONFERENCE OF THE URBAN HISTORY GROUP AT GILBERT MURRAY HALL, UNIVERSITY OF LEICESTER, ON 23–26 SEPTEMBER 1966. Edited by *H. J. Dyos*. (London: Edward Arnold. 1968. Pp. xxii, 400. 90*s*.)

SAM Bass Warner, Jr., in his article, "If All the World Were Philadelphia" (*AHR*, LXXIV [Oct. 1968], 26–43), deplores "the tradition of local history that has long stressed the distinctiveness of each urban portrait" as preventing "historians from considering the comparative and sequential aspects of urban environments." This volume provides the record of a conference designed to see what could be done about just that problem: how to pursue the comparative study of urban phenomena in a scientific manner, without distorting what is unique and special in individual towns. Dr. Dyos does ask, "Is it preferable that we should concentrate, after all, not on the general pattern of the historical process of urbanisation, but on particular cases?" But on the whole the tendency of the conference, composed for the most part of historians and social scientists, is to call for a greater emphasis on a search for the general and the typical. Differing from the symposium on the Victorian city at Indiana University in March 1967—papers from which have been appearing in *Victorian Studies*—in the small number of participants from literature and the arts, the conference shows a preference for "hard," quantifiable data and for uniform categories of investigation that would facilitate comparative studies.

Although much of the volume deals with the British nineteenth century, the emphasis on problems of methodology renders it valuable to anyone working on urban history in any time or place. The papers are of high individual quality.

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Asa Briggs has contributed a brief foreword, and there are useful bibliographical essays by Dyos and François Bedarida. W. A. Armstrong, Dyos, and A. B. M. Baker discuss the problems connected with using computers to analyze census records. M. R. G. Conzen suggests how historians can make more profitable use of town plans. F. H. W. Sheppard explains the policies being followed in editing the *Survey of London*. G. H. Martin and Francis M. Jones give examples of the different ways that visual evidence can be used in interpreting the history of towns. Leo Schnore gives an account of the present state of urban sociology, particularly in its ecological aspects. J. D. Marshall describes the creation of new industrial communities in nineteenth-century Lancashire, while Harold Carter gives an account of the less successful efforts to create new towns in medieval and early modern Wales. D. A. Reeder deals with Victorian suburban development in Paddington, Kensington, and Hammersmith. John Foster tries to account for the varying degrees of class consciousness in nineteenth-century Oldham, Northampton, and South Shields. There are two further studies of the changing relations between social class and politics in the nineteenth century: for Exeter by R. Newton; for Leeds and Birmingham by E. P. Hennock. S. G. Checkland examines the dilemma of the historian in deciding whether to put his city in the context of other cities, or in the context of its time and place.

The general discussion following each group of papers has been preserved in a summarized form. In many ways the discussions are the best part of the book; they give, in a lively yet lucid fashion, a sense of the problems urban scholars are grappling with today. The leaders of the discussions did a superb job of tying together specific material from the papers, isolating problems that were troubling participants, and suggesting fruitful new approaches. The common concern with methodology gives coherence and unity to the whole volume.

One recurring worry of the participants is the anarchic tendency of individual historians to define their subjects and pursue their research using their own criteria and vocabulary irrespective of their compatibility with those of other scholars in their field. On the other hand one might question the wisdom of undertaking examinations of "urbanisation" or "the industrial city" until we know more about particular towns in specific periods. Is there not danger, in adopting a uniform set of questions to be answered, of blurring important distinctions and encouraging the historian to overlook evidence that may not fit his criteria? London, to take an extreme example, may well require a special set of tools applicable to it alone; no other British city, and certainly no foreign one, is even remotely like it.

But the advantages of the quantitative approach, and the usefulness to the urban historian of the techniques of the social sciences, have rarely been more persuasively set forth, the more so because this is not a polemical work but rather an opportunity to overhear an extended and lively debate. We need more such conferences, more publications that recapture so successfully the spirit of the encounter.

Vassar College DONALD J. OLSEN

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CONJECTURE, PROPAGANDA, AND DECEIT AND THE SPANISH CIVIL WAR: THE INTERNATIONAL CRISIS OVER SPAIN, 1936–1939, AS SEEN IN THE FRENCH PRESS. By *David Wingeate Pike*. ([Stanford, Calif.:] California Institute of International Studies. 1968. Pp. xxii, 317. $4.00.)

THE theme of this book, published by Professor Ronald Hilton's study institute in California, is defined by its subtitle, for the main part of the volume consists of a systematic study of political opinion, pressures, and propaganda in the French press concerning the Spanish Civil War. The most original aspect, however, was made possible by access to provincial archives of the French Pyrenean districts, providing Pike with the first direct documentation on French military assistance to the Spanish Left both before and after the signing of the Nonintervention Agreement. It concludes with an interesting chapter on Spanish refugees in southwestern France at the close of the Civil War. The book is weaker when referring directly to events in Spain, which the author understands less well than those in France. This has led him to such curious comparisons as the idea that the "French experience" of 1931–1936 was "not dissimilar" to that of Spain during the same years.

Though more friendly to the Left than to the Right, Pike largely sustains an objective and scholarly approach. His investigation is, on the whole, careful and judicious, and it contributes to our knowledge of the international dimension of the Spanish Civil War.

University of Wisconsin, Madison STANLEY G. PAYNE

THE NEW NATIONALISM. By *Louis L. Snyder*. (Ithaca, N. Y.: Cornell University Press. 1968. Pp. xiv, 387. $11.50.)

IN undertaking this book Louis Snyder has set himself the challenging task of surveying and analyzing the development of the "new nationalism," making 1945 the breaking point between old and new. He makes no claim that postwar nationalism, which he sees as becoming for the first time the political creed of all mankind, is radically different from that which preceded it, but he contends that it has introduced new and characteristic variations on an already complex multiplicity of earlier forms. His concluding paragraph restates the "persistent theme" that *The New Nationalism* illustrates and elaborates: "the nation-state remains the chief political instrument of the world; nation-preserving and nation-building remain the major political pursuits of man."

In structure the book consists of four introductory chapters that define the problem and seek to identify the new nationalism by comparing and contrasting it with what went before. The fourth of these chapters proposes a classification of nationalisms on a continental and regional basis that establishes the framework for most of the book. Each area is given its own chapter, and the headings indicate both the nature of the approach and its dangers and pitfalls: Europe is labeled as fissiparous nationalism; Africa, black nationalism; the Middle East, politico-religious; Asia, anticolonial; Latin America, populist; the United States, melting pot; the Soviet Union, messianic.

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Snyder has bravely aimed at universal coverage, but what can actually be dealt with in some 370 pages of text is of necessity sharply limited. Thus, in the chapter devoted to Asia, India and Indonesia are each allotted seven pages; China, eight; Japan, ten. Such capsule summaries lend themselves all too easily to oversimplification and perhaps to distortion.

We have much to be grateful for in a volume that embraces such a vast domain. Beyond question, merely bringing together this wide range of diverse material on a comparative basis into a single book is a stimulating venture and inspires a rethinking of established positions and interpretations. Although there is no bibliography, the footnotes provide a treasure of writings on nationalism, and important segments of the literature are discussed in the text.

When this has been said, however, it is unfortunately necessary to add that the speed with which the globe has been traversed has forced the author not only to pick and choose somewhat arbitrarily in the realms of relevant human experience but also to venture statements and judgments of dubious validity. A random few of these might be cited to indicate that readers must proceed somewhat cautiously. It is said of the Congo that Katanga seceded in 1962–1963 and that full-scale civil war in 1964 led to intercession by the United Nations; whereas Katanga actually seceded in 1960, and the United Nations withdrew its forces in 1964, despite the civil war and after Katanga had been brought back into the fold early in 1963. While on page 113 white colonists Balkanized much of Africa along tribal lines, on page 137 the colonial boundaries were drawn by Europeans who ignored tribal lines. Can it really be said that Marx predicted a union of nationalism and socialism that emerges from the dialectic as a synthesis called National Socialism or fascism, and did Stalin really regard the very existence of non-Russian languages as dangerous? In a book specifically dealing with nationalism, can Nigeria be called a nation, and can the new African states be seen as having achieved nationhood? Nations are surely not to be equated with states on so casual a basis.

Harvard University RUPERT EMERSON

ABSCHRECKUNG ODER VOLKSKRIEG: STRATEGISCHE MACHTPLANUNG DER SOWJETUNION UND CHINAS IM INTERNATIONALEN KRÄFTEVERHÄLTNIS. By *Helmut Dahm*. (Olten: Walter-Verlag. 1968. Pp. 414. 35 fr. S.)

DER CHINESISCH-SOWJETISCHE GRENZKONFLIKT: DAS ERBE DER UNGLEICHEN VERTRÄGE. By *Horst Pommerening*. (Olten: Walter-Verlag. 1968. Pp. 266. 26.50 fr. S.)

IN the past ten years the split between the two giants of the Communist world has inspired a steadily increasing list of studies of Sino-Soviet relations. These books recently published by Walter-Verlag not only add two new titles to this list; they also supplement the existing literature on the conflict between Soviet and Chinese military strategy and the historical background of the Sino-Soviet border dispute.

Helmut Dahm has written an accurate, detailed, but often dry account of Soviet and Chinese military doctrines. He examines both the political and strategic

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factors that have influenced the development of military policy in the two countries and concludes that the differences in doctrine are largely a function of different levels of technological and military development, as well as political goals. The author discusses at great length the evolution of Soviet military doctrine through various stages since Stalin's death, including the development of the concepts of "preventive war," deterrence, and different types of warfare. Dahm also traces the changes in Chinese military strategy since 1945 and emphasizes the importance of the cultural revolution for contemporary Chinese doctrine, as well as the relationship of the cultural revolution to the policy of wars of national liberation.

The second half of the book contains translations of two articles by Marshall V. Sokolovskii and General M. Cherednichenko on Soviet strategy and articles by Lo Jui-ch'ing and Lin Piao on Chinese strategy. An excellent bibliography of both primary and secondary materials on Soviet and Chinese military doctrine completes the study.

Abschreckung oder Volkskrieg follows in the tradition of studies of Soviet strategy by Herbert S. Dinerstein and Raymond L. Garthoff and those on Chinese military strategy by Morton H. Halperin and Alice Langley Hsieh. But, besides bringing the discussion up to date, it also adds material for a comparison of the strategies of the two Communist world powers.

While Dahm deals primarily with recent differences between the Soviet and Chinese leaderships, Horst Pommerening traces the development of the boundary dispute between the two countries since the initial contacts between Russians and Chinese in Central Asia in the sixteenth and seventeenth centuries.

Pommerening first discusses the present position of the border dispute and then turns to a legalistic analysis of Western, Soviet, and Chinese views on unequal treaties. Although the discussion of unequal treaties is relevant to his analysis of Chinese claims on Soviet territory, the author devotes too much space to the subject relative to the length of the book.

One of the most interesting sections of the study is the excellent treatment of traditional Chinese attitudes toward other states and the absence in Chinese thought of the Western concept of a border as a dividing line between sovereign states. Pommerening points out that the present Chinese view of a world under Mao's leadership fits into the traditional framework of Chinese thought: "Under the Chinese Republic of 1911, under Sun Yat-sen, under Mao Tse-tung the view was maintained that it was the duty of China to educate and lead weaker peoples and to help them achieve an independence that could, however, never turn against China."

After discussing traditional Chinese attitudes toward foreigners, the author presents an outline of Chinese-Russian relations during the last three centuries and of the gradual Russian encroachment on Chinese-controlled territories. The Chinese now argue that most Russian territorial gains resulted from unequal treaties that have no legal basis. They were merely the result of Russian imperialism and should be returned to China.

The Soviet defense is based largely on power considerations. According to Soviet authors, the areas in the Amur Valley in the Far East were seized from Russia in the seventeenth century and reacquired two hundred years later. Second, Russian imperialism was inherently more progressive than the Chinese variety

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and therefore justifiable. A third argument is related to the principle that "possession makes it legal." Since Russians have occupied Central Asia for a century and have developed the area, they cannot be expected to turn it over to China. Finally, the Soviets argue that all border disputes must be solved peaceably, lest they lead to destruction in a nuclear war.

Both China and the Soviet Union have shown that they are unwilling to apply the principles of proletarian internationalism to the dispute and that an early resolution of the conflict is very unlikely.

Both of these books show quite clearly that the present dispute between the Soviets and the Chinese is not merely a difference in interpretations of Marxist-Leninist doctrine. Concrete power considerations play a significant part in the dispute. For the student of Chinese-Soviet relations, both of these studies are important additions to the literature. For the more casual reader, however, Pommerening's study of the border conflict should prove less tedious and more rewarding.

University of Kansas ROGER E. KANET

Ancient

EUROPA: STUDIEN ZUR GESCHICHTE UND EPIGRAPHIK DER FRÜHEN AEGAEIS. FESTSCHRIFT FÜR ERNST GRUMACH. Edited by *William C. Brice.* (Berlin: Walter de Gruyter & Co. 1967. Pp. xi, 349, 28 plates. DM 120.)

THIS sixty-fifth birthday offering for the East German scholar Ernst Grumach became, sadly, a memorial volume upon his death in 1967. W. C. Brice, the editor, selected a good range of essays that are, as in all *Festchriften,* of uneven quality, and stronger in epigraphy than in history despite the title. They do, however, offer some new material and sensible observations.

Grumach had long been interested in Aegean languages and writing. He disagreed with Michael Ventris' decipherment of Linear B as Greek on both historical and linguistic grounds, but he was open-minded about it and established the journal *Kadmos* for publication of diverse views on early scripts and difficult languages. Most of the linguistic essays here spring from Grumach's position. They offer notes on structure, analogy, abbreviations, ligatures, ideograms; none offer a positive alternative to the Ventris values. F. R. Richards makes an interesting though hostile analysis of Ventris' thought processes as he developed the Linear B grid. Apparently he thought Ventris was filled with unconscious drives toward preconceived patterns and leaped into areas where "imagination has outstripped judgement." A. J. Beattie typically uses Ventris' values to show that "there is nothing Greek" in the deciphered language. K. Ktistopoulos looks to the Ugaritic archives to explain "non-Greek" names in Cretan tablets, yet he emerges comparing Ugaritic *prtn* to Plato. The reader is rescued from confusion by D. Young, who explains that Linear B can be (was?) used for any language and offers alternative translations for the notorious "Aigeus the Cretan" tablet, *tiripode aiķeu ķeresijo weķe* TRIPODS 2: "stripholder Aingeus Grayson, whelker: Pot-

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stills 2" for an enterprising Gaelic distiller, or, if Greek, "Aischeus has an outhouse full of woodworms."

Among the articles that range beyond technical discussions of Linear B, L. H. Jeffery makes a valuable survey of how later Greeks thought that writing began. P. Meriggi and O. Masson tackle Carian, both good studies; R. F. Willetts explores archaic race and language in Crete. P. Faure must interest both historians and archaeologists as he locates the small place names and landscape features bounding the ancient polis of Lato. M. Cameron continues with valuable joins in the Knossos frescoes, extending and correcting older views of garden and monkey scenes; one hopes his eighth note, on the impropriety of traditional methods of fresco conservation, will eventually be taken to heart by Greek archaeologists. M. S. F. Hood traces intrusive Balkan pottery at Mycenae, which is similar to that found at Troy after its destruction, and suggests a northern raid on Greece in the thirteenth century. Sp. Marinatos theorizes that Linear B developed because scribes from eastern Crete, fleeing to Greece in fear of the eruption of Thera, "introduced the bureaucracy of Minoan palaces and organized archives" in places like Pylos. How they did this in the fifteenth century when the palace was not built until the thirteenth is not clear. J. T. Hooker suggests sensibly that Linear B grew up both in Crete and on the mainland, moving slowly, locally, and tentatively through phases of development.

In short, this is a characteristic *Festschrift,* with slight but often interesting articles; it is, on the whole, objective and certainly of interest to professionals and graduate students. Undergraduate libraries may, however, find it expensive for what it offers.

Wellesley College EMILY VERMEULE

CITIES AND NATIONS OF ANCIENT SYRIA: AN ESSAY ON POLITICAL INSTITUTIONS WITH SPECIAL REFERENCE TO THE ISRAELITE KINGDOMS. By *Giorgio Buccellati.* [Studi semitici, Number 26.] ([Rome:] Istituto di Studi del Vicino Oriente, Università di Roma. 1967. Pp. 264. $7.50.)

THIS book consists of three parts, of which the third, and by far the best, is a careful analysis of the political structure of the Israelite kingdoms, which thoroughly refutes Alt's theories as to the charismatic nature of Israelite kingship and the peculiar political status of Jerusalem and Samaria. This part is well done and should be definitive.

In contrast, the first section tries to describe the forms of polity in the Syrian states of the second millennium B.C. and to locate, in relation to these, the polity of Israel. It distinguishes between "territorial" and "national" states: in the former the people think of themselves as inhabitants of a given territory; in the latter, as members of an ethnic group essentially independent of the land it happens to acquire. By these definitions the Bronze Age states of Syria turn out to be "territorial"; the nomads and some of the states they set up, notably Israel, Edom, and Moab, were "national." Considerable ingenuity is shown in deriving from these "essential" differences the actual differences to be observed between these states, and the contrast of the imaginary "pure" types is instructive. But in

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real life the types were almost always mixed, and, in any case, the political and social conditions of the Bronze Age differed so far from those of the Iron Age, half a millennium later, that Buccellati is forced to admit that his categories will not fit the non-Israelite states of the latter period. The contrasts he describes, therefore, are largely the consequences of this latent anachronism.

From its ideal picture of the "national" polity of the nomads, the second part of the book goes on to explain the origin of the Israelite kingdoms. It follows Noth's theory of a league of tribes (though objecting to the term "amphictyony"). But the league theory does not fit the political theory that the unified national state grew from "an" original nomad group. (Note, on page ninety-four, the jump from plural to singular.) Therefore the argument suffers, even more than Noth's, from the absence of evidence for union in early Israel; its attempts to demonstrate the existence of a union, by the way they have to rely on redactional material and on the Chronicler(!), instead prove the opposite.

It is fortunate that the book is a work of history rather than of logic; the anachronism of the first part and the credulity of the second do not seriously detract from the brilliant success of the third.

Columbia University MORTON SMITH

A HISTORY OF SPARTA, 950–192 B. C. By *W. G. Forrest.* [Classical History and Literature, Number 125.] (London: Hutchinson University Library. 1968. Pp. 160. Cloth 27*s.*6*d.*, paper 11*s.*6*d.*)

LIKE Etruscology and Pentateuchal criticism, the history of Sparta is a topic beset with controversy because the evidence is fragmentary and often contradictory. In this admirable but too brief book, W. G. Forrest tackles the main problems of Spartan history with erudition and imagination. His literary style is buoyant and laced with wit: Plataea, for example, "was a brave but untidy victory, won as even Athenians conceded by 'the Dorian spear,' but with no great display of Dorian brains." Because of its brevity, the book lacks classical references, but it does include useful bibliographical notes.

Although he admits to a "natural distaste for Sparta and its generally so unattractive citizens," the author is sufficiently Hellenophilic to praise their tardy defense of Greece in the Persian War; he also admires the social reforms of the Hellenistic radicals, Agis, Cleomenes III, and Nabis. While properly skeptical of Spartan traditions, he nevertheless reconstructs the careers of early kings and other developments with more certainty than the evidence necessarily warrants.

As for the details of early Sparta, Forrest sees a connection between the conquest of Messenia and the Lelantine War; he interprets the "crooked decisions" of Plutarch *Lycurgus* 6.4 as meaning that the assembly could not amend proposals from the Gerousia, which was otherwise bound to accept the vote; he suggests that Alcman lived after the Lycurgan reforms and that the *syssition* contained not fifteen but maybe three hundred members; and he dates the Lycurgan reforms about 676 B.C. when the first Carneia was supposedly held and Kings Polydorus and Theopompus were both reigning. Forrest may well be correct in these contentions, but I consider stories about good King Polydorus as dubious as those about the

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sainted Lycurgus. The institution of the Lycurgan discipline was posited on a crisis, and the disorders of the Messenian revolt were likely its cause; Herodotus 1.65 even dates the change in Spartan life about the time of Leon and Agasicles (about 570 B.C.). Since Athenians of the fifth century were confused about Solon and even the events of Hippias' reign, Spartan notions of the antiquity of the Lycurgan system should not be given credence, and it is noteworthy that they changed from generation to generation.

According to Forrest, "Xenophon (or his text) is simply wrong in the number of Spartans he gives for Leuctra," but Forrest too is wrong when he says that Cleomenes III "faced the last campaign of 222 with an army of 10,000." Polybius 2.65.7 says twenty thousand (Walbank, *Commentary* 1.279). It is regrettable that Forrest accepts as authentic the "letter" of Areus I (I Macc. 12.20–23) in which the King admits the claim that the Spartans are children of Abraham! This obvious Jewish forgery, which is found in a letter from the wily Hasmonaean Jonathan, is a typical Hellenistic production and comparable to Hiempsal's ludicrous claim that the Numidians were Persians and the Moors Medes (Sallust *B.J.* 17.7–18.11).

Despite these points of disagreement, I consider this a valuable and stimulating book. Forrest's emphasis on the "megalomania" of the Spartan myth is very perceptive, and I commend his psychological interpretation of the strange life of Cleomenes I. Although there are obvious dangers in psychoanalyzing dead men, Forrest's view of Cleomenes' development and behavior is convincing, and more classical scholars should have the imagination to follow his lead in using psychology to explain some of the problems of the past. Modern scholars have long employed economic and sociological insights in the study of ancient history, and, when the evidence warrants, psychology can also supply informed guesses on human motivation.

University of Southern California THOMAS W. AFRICA

GREEK FEDERAL STATES: THEIR INSTITUTIONS AND HISTORY. By *J. A. O. Larsen*. (New York: Oxford University Press. 1968. Pp. xxviii, 537. $14.40.)

THE town hall democracy that the Athenian city-state left the afterworld as one of its notable legacies has been the subject of many rewarding studies. Not so commonly known is the Greek experience with the federal state, a form of government more pertinent to our own times. The nonspecialist will be surprised to realize that many (about fifteen are listed here) such confederacies existed in the Greek world; they developed mainly from tribal and ethnic groups (Boeotians, Achaeans, Aetolians, and Arcadians), but were by no means confined to them.

The task that Professor Larsen performs for us in this first comprehensive survey of the subject since E. A. Freeman's *History of Federal Government in Greece and Italy* (1863) is a formidable undertaking, since, as the author points out, there is no ancient description of a confederacy paralleling Aristotle's *Constitution of Athens,* or a discussion of the theory of federalism, as there is of the city-state in Aristotle's *Politics*. The picture of the federal state must be drawn

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from "odds and ends in literary sources" (mainly Polybius) "and inscriptions." This demands the most thorough conversancy with Greek history, great acumen in the perception of what is and how it is relevant, as well as restraint in drawing the line between what is likely and reasonable and what is purely conjectural. The author demonstrates these qualities on every page of the work. The treatment is characterized by clarity and specificity, with a refreshing absence of vague generalization, albeit with some repetition of detail and considerable, but reasoned, conjecture.

In its rudimentary form the federal state was characterized by *sympoliteia,* that is, common citizenship of several city-states or of several related tribes in which the armed forces were supported by the revenues from member states and foreign affairs were in the hands of the federal government. The further development of federalism is a study of the nature and extent of the jurisdiction of the federal government, side by side with the jurisdiction of the member political unit in matters concerning the rights and privileges of *sympoliteia,* the imposition of taxes, the levy and use of the armed forces, the make-up of the assemblies and of the courts, the passage of legislation and the conduct of internal administration and of foreign affairs, and, specifically, how the jurisdiction is exercised through the member units or through federal agencies, by primary or by representative assemblies. One might question whether there is sufficient justification for including in the category of federal states some of the confederacies here considered such as, for example, the Euboean and Italiote Confederacies.

The treatment of federalism before the King's Peace (Part I) is followed by a study of the part played by federal states in the history of Greece up to that point; Part II, "Federalism after the King's Peace," is followed by a similar treatment of the federal states vis-à-vis Macedonia and Rome.

A select bibliography, a full and accurate general index, and a partial list of the literary sources and inscriptions bearing on the problems discussed add to the value and the usability of this excellent work.

University of Texas, Austin O. W. Reinmuth

MEN AND GODS ON THE ROMAN NILE. By *Jack Lindsay.* (New York: Barnes and Noble. 1968. Pp. x, 457. $11.00.)

This is Jack Lindsay's third book dealing with Roman Egypt. Like the earlier two, this work is a popularization of the cultural history of Roman Egypt based mainly on the evidence provided by the vast number of papyri, literary and non-literary, which have survived until modern times. The central points of focus are the river Nile, itself, the activities that took place both on it and beside it, and its impact on the emotional consciousness of the inhabitants of the land through which it coursed. Much of the book concentrates on the varieties of religious experiences and cults flourishing in Egypt at this time. Lindsay fully and rightly understands that Roman Egypt's culture had deep roots in the culture of Pharaonic Egypt and that the element of continuity between the two was deep, strong, and visible. Roman Egypt was more than just a continuation of Pharaonic Egypt, however; the intervening periods of Persian and Greco-Macedonian rule caused

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change and interaction, and the author also concerns himself with an investigation of the effect of Egyptian ideas on Greek and subsequently Roman ideas. The last two chapters, indeed, are devoted to one aspect of this in respect to the Egyptian affiliations with the Greek legend of Perseus and Andromeda. The book is a popular work, but the specialist may use it with profit. Lindsay has a pleasant, readable style, and his scholarly credentials are respectable.

Queens College, Flushing, New York ALAN R. SCHULMAN

RZYMSKIE ZGROMADZENIE WYBORCZE OD SULLI DO CEZARA [The Roman Electoral Assembly from Sulla to Caesar]. By *Jerzy Linderski*. [Polska Akademia Nauk, Oddział w Krakowie, Prace Komisji Nauk Historycznych, Number 16.] (Cracow: the Akademia. 1966. Pp. 173. Zł. 33.)

THE object of this pioneering venture in Polish studies of ancient history is to investigate in the scope of seven chapters the functioning of Roman electoral procedures during the late republican period from the time of Sulla to the assumption of power by Julius Caesar in 49 B.C. In undertaking this task, the author has been impelled by the belief that, owing in part to the fragmentary nature of sources relating to the electoral processes of this thirty-year period and in part to an imperfect understanding of the records that have survived the intervening centuries, a restudy of the records is essential to a proper understanding of the procedures of the Roman electoral assemblies. Thus Linderski gives his own interpretations of the relevant factual sources and indicates where his own views differ from those previously regarded as authoritative. He defines, for example, the various types of Roman electoral assemblies and gives considerable attention to voting procedures. Here, as elsewhere, the author is put to the test as an interpreter; inasmuch as the original accounts are not only incomplete, but conflict on some points, they challenge the historian to determine *wie es eigentlich gewesen*. Conflicting accounts lead to the conclusion that in some circumstances custom superseded the provisions of electoral law. The author challenges existing conceptions concerning the holding of elections for the centuriate and tribal assemblies and maintains, in contrast to Theodor Mommsen, Lily R. Taylor, and T. R. S. Broughton, that the right to preside over elections was a prerogative of the consul prior. He differs with Mommsen in concluding that announcement of candidacy could occur after the announcement of elections.

The reasons given by the author for his conclusions in areas of possible doubt certainly constitute the most valuable feature of the book. While much of the work is analytical, certain fundamentally expository sections deserve commendation. Among these is the final chapter, dealing with the decline of the electoral assembly, which Linderski attributes, among other things, to the imperfection of the electoral system. It is an interesting introduction to the transitions associated with Caesarian rule.

In spite of Taylor's brilliant treatment in *Roman Voting Assemblies from the Hannibalic War to the Dictatorship of Caesar* (1966) and Linderski's concentration on the shorter period, many problems remain unsolved.

Library of Congress JANINA W. HOSKINS

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BRITAIN IN THE ROMAN EMPIRE. By *Joan Liversidge*. (New York: Frederick A. Praeger. 1968. Pp. xxxiv, 526, 60 plates. $17.50.)

THE EXCAVATION OF ROMAN AND MEDIAEVAL LONDON. By *W. F. Grimes*. (New York: Frederick A. Praeger. 1968. Pp. xxi, 261, 102 plates. $13.50.)

ALTHOUGH Joan Liversidge and W. F. Grimes are both well qualified to write on the archaeology of Roman Britain, their books are very different in aim and accomplishment. Liversidge's *Britain in the Roman Empire* aims at a wide audience and seeks to present as complete a picture as possible of daily living in Roman Britain. What results is not a history of Roman Britain, for the approach is topical; a chronological table is, however, supplied.

Liversidge bases her presentation primarily on British archaeological evidence, but since that alone would not produce a comprehensive account of everyday life, she turns readily to continental sites and to literary sources for additional information. Her application of this alien material to a reconstruction of British life sometimes appears strained, particularly in the chapters on the professions. An example is the discussion of education, which consists of a general description of the Roman system of education and is followed by an account of schools in Gaul, all of which is justified by the statement, "With so much evidence for a lively intellectual life in Gaul it is difficult to believe that some of this activity did not cross the Channel and the North Sea." When Liversidge assesses the material evidence in Britain, she finds little more than some graffiti scratched on tiles.

Britain in the Roman Empire seems a not wholly successful compromise between a detailed description of archaeological findings that would interest only specialists and a "daily life in . . ." approach that would have wide appeal. The general reader will become bogged down in detailed descriptions of objects, while the expert will be bored by already familiar descriptions of aspects of Roman civilization. Yet the book does accomplish something. It contains careful descriptions of buildings and art objects accompanied by accurate plans, figures, and plates. Any student of Roman art and architecture will find use for such a comprehensive survey of the archaeology of Roman Britain.

W. F. Grimes's *The Excavation of Roman and Mediaeval London* is much narrower in its aim, although its achievement may be greater. It is a preliminary record of excavations made within the old walled area of the City of London by the Roman and Mediaeval London Excavation Council, from 1947 to 1962. Wartime bombings cleared blocks in the heart of London, unexpectedly opening them up for archaeological investigation. Grimes's book has a double aspect: his account of the problems of excavation in London and their solution and his description of the sites and the finds. Naturally, these sites in the heart of a great city busily rebuilding its wartime damage presented peculiar problems, which make the book especially interesting to students of archaeological techniques. Many of the sites were filled with rubble and leveled to make parking lots. Grimes and his council had to secure public support for the excavations. Although he generally accepts the difficulties with good humor, in his epilogue he is unable to mask his disappointment at the inadequate support of the London Corporation.

The council's excavations revealed both Roman and medieval remains, for

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on continuously occupied sites the layers of deposits became much intermingled. Over half the book, however, is devoted to Roman remains because they lay deeper and were less likely to be disturbed by the growth of the city above. The most spectacular finds were Roman, including Cripplegate Fort and the Mithraeum. Medieval excavations include the sites of several famous old City churches. The descriptions of the sites are illustrated by many drawings, mostly the work of the author, as are most of the 102 photographs. Grimes has benefited all students of British archaeology in summarizing the results of the excavations in London.

Ohio University RALPH V. TURNER

BYZANTIUM AND THE DECLINE OF ROME. By *Walter Emil Kaegi, Jr.* (Princeton, N.J.: Princeton University Press. 1968. Pp. xi, 289. $10.00.)

THIS book attempts to assess the reactions of the Eastern Roman (or Byzantine) Empire to the collapse of the Western Empire during the fifth century A.D. The work is clear and very well documented, as we should expect from the pen of one of the best of the younger generation of American Byzantinists and a pupil of Professor R. L. Wolff. Kaegi begins by examining the effects in the East of the traumatic experience of Alaric's capture of Rome in 410. These were not very noticeable (St. Augustine's "plangentibus orientalibus populis" is not to be regarded as history) compared with those felt in the West. The only prominent Eastern resident who took it to heart was St. Jerome, and he was more Latin than Eastern. We look in vain at Byzantium for a reaction like that of St. Augustine or Orosius. Theodosius II did indeed send four thousand men to help Honorius against Alaric, but even this token force was not used. Numismatic evidence certainly proves that, officially speaking, the East was concerned to preserve the façade of a united empire under two emperors. But after the accession of Zeno (474) and the deposition of Augustulus (476), no further help for Italy was forthcoming until the invasion by Justinian. After that, until the time of Manuel Comnenus, Byzantine emperors never forgot the West, but the theory of a Western half of empire, with an independent partner on its throne, was discarded. Byzantium wanted a West governed by it, and Charles and Otto were never recognized as anything but disagreeable and temporary expedients.

Instead, the disasters that befell old Rome were used by religious polemicists to prove the righteousness of their cause. Kaegi's account of their arguments, if they are to be called that, is everywhere delightful, and very frequently hilarious. The pagans accused the Christians of bringing on the debacle by neglect of the gods. The Christians accused the pagans. The Christians, of various colors, accused one another. None of them, whether Christian or pagan, unless it were Zosimus, had the smallest claim to being a historian in the modern sense of the word. Thucydides would have ridiculed them. The most vivid and valuable part of Kaegi's book is his brilliant and penetrating analysis of Zosimus, whose weaknesses, and comparative strength, are excellently demonstrated.

After surviving its Germanic crises in the fifth century, the Eastern Empire actually moved into a period of prosperity, during which time it regarded the calamities of the West with some *Schadenfreude*. Arcadius had no compunction

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in turning Alaric to Italy, and the part played by Zeno in the similar migration of Theodoric is well known (Procopius, *B.G.* 1.1.10). The only Western area about which Byzantium cared at all was Africa. The reason is plain: Africa led to Egypt and, ultimately, to Syria.

The only fault, I venture to hint, with Kaegi's excellent presentation is the lack of a section on the fundamental and lasting disunion between Latins and Greeks. There were, no doubt, linguistic, ecclesiastical, and commercial differences. But the fundamental cleavage, from the time of Juvenal to the time of Constantine XI and after, ran deeper; it never healed. Kaegi himself points to the miscalling of Anthemius in the West as "graecus imperator." "Graeculus," as Mr. A. N. Sherwin-White has recently reminded us, "is the Latin for 'wog.'"

Dumbarton Oaks R. JENKINS

Medieval

THE MIRROR OF LANGUAGE: A STUDY IN THE MEDIEVAL THEORY OF KNOWLEDGE. By *Marcia L. Colish*. [Yale Historical Publications, Miscellany 88. Published under the direction of the Department of History.] (New Haven, Conn.: Yale University Press. 1968. Pp. xxiii, 404. $10.00.)

THE author's purpose is to show how the three parts of the trivium of the medieval schools (rhetoric, grammar, logic) and poetry (an addition to her original dissertation) shaped the theology of Augustine, Anselm, Aquinas, and Dante, respectively. As Christ was the Divine Word incarnate, so the human word was the primary sensuous means of formulating and communicating knowledge of God. Thus, for Augustine, followed by the others, spiritual reality could be made known by language that truly corresponded to the thing signified. Such knowledge was partial, seen *per speculum in aenigmate* (through a glass darkly). Its truth was recognized because of prior belief derived from faith. For believers it was, therefore, "commemorative" and "intensive" of what they already knew.

Relying on this theory as a common basis, Miss Colish examines the epistemological positions as to theology of the four writers. Her aim is intellectual history, not philosophical evaluation. Her method, supplemented by an extensive command of modern commentary and controversy, is careful, analytical summary of the original documents. There is probably too much summary, especially when, as in the case of Dante, some of it has little relevance to the theme. She uses her critical flair persuasively, however, in establishing the views of the theologian and the poet distinctively within the setting of the appropriate mode of linguistic usage. Broader historical factors are reviewed briefly.

Her treatment of Anselm and of Aquinas illustrates the novelty and aptness of her method, along with the problem of making critical judgments relative to the historical setting. Anselm, the grammarian, through definition of terms and use of equipollent, or equivalent, propositions, in her view, avoided the weakness of the syllogistic form of the ontological proof of God's existence, whereas St. Thomas is criticized for misconstruing Anselm's grammatical argument in condemning it from a logician's viewpoint. She is, at the same time, unsympathetic toward Thomas' proofs of God from the evidence of nature. His "true in-

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tention," she argues, was to show their "inadequacy" in comparison with the evidence of faith. Her ambivalence about his "deep-rooted" empiricism and his logic reflects her unhappiness over his blending of Aristotle and Augustine. She prefers the stricter a priori reasoning of Anselm.

Figurative language, the verbalization of "nonverbal" signs in allegory and metaphor, is well introduced in the chapter on Augustine's "redemption" of classical rhetoric for Christian eloquence. The original plan of the book is complete without the lengthy chapter on Dante. He is an uneasy partner of the theologians and could have stood alone. The book makes its essential points without him.

Reed College R. F. Arragon

MEDIEVAL PARLIAMENTS: A COMPARATIVE STUDY. By *Antonio Marongiu*. Translated and adapted by *S. J. Woolf*. With a foreword by *Helen M. Cam*. [Studies Presented to the International Commission for the History of Representative and Parliamentary Institutions, Number 32.] (London: Eyre and Spottiswoode; distrib. by Barnes and Noble, New York. 1968. Pp. 306. $11.25.)

Writing a survey of the development of medieval parliamentary assemblies in Europe is still a hazardous business. Vast erudition is required, as well as exceptional judgment. Professor Marongiu conspicuously possesses the former, but not quite so conspicuously the latter. He has put forward some interesting and important theories, but he has not given them the systematic and lucid discussion they deserve.

His most important ideas would seem to be his distinction between "pre-parliaments" and "true parliaments" and his attempt to define their respective attributes; his discussion of the relative importance of theory and practice in the transition from the one to the other; his emphasis on the importance of the great Spanish assemblies in the general growth of the parliamentary institution; and his attempt to illuminate the complicated problem of the English assembly as it is affected by recent research.

Of these ideas, his discussion of Spanish and Italian parliaments will be gratefully received by all those unfamiliar with his earlier writing and with modern research in these fields. His survey of the transition from feudal assemblies to the type of parliament referred to by Humbert de Romans and Walter Giffard in the thirteenth century is quite exceptional in its breadth if at times it is not easy to follow. But his discussion of theory and practice adds very little to the extensive literature already existing, and his incursion into the controversies and intricacies of English parliamentary history may even be described as a calamity. Here, Marongiu's exposition is marked by an abundance of error. He tilts against the views of Richardson and Sayles, as do many English-speaking writers, but shows himself hardly qualified for such a melee. His discussion of the Model Parliament of 1295, of the *Confirmatio cartarum* of 1297, of the Statute of York, and of other problems shows how fragile are his weapons and how unfamiliar this terrain.

One appealing quality that is claimed for the book is that it is a comparative study, but this is true only to the extent that the histories of the different medieval

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parliaments are given side by side. This appears even in the discussion of the two great complementary institutions of England and France, which were so intimately connected and so divergent in their growth. The Parlement of Paris finds no significant place in the discussion of the French assembly, and suggestive modern studies like those of Fawtier and Langmuir are apparently ignored.

It is hard not to receive such a brave and learned volume with both gratitude and admiration. But the author seems to have gone out of his way to invite criticism. Even his bibliography leaves much to be desired. The process of selection must have been an almost impossible task, but even this does not altogether excuse the strangest inclusions and omissions. Professor Cam's foreword is restrained, but still raises expectations unduly. Dr. Woolf's translation is one of the most attractive features of the book.

University of British Columbia B. Wilkinson

AFRICANOBYZANTINA: BYZANTINE INFLUENCES ON NEGRO-SUDANESE CULTURES. By *Theodore Papadopoullos*. [Πραγματεῖαι τῆς Ἀκαδημίας Ἀθηνῶν, Number 27.] (Athens: the Akademia. 1966. Pp. xiv, 155.)

This book examines an obscure and neglected subject, that of Christianity in Nubia and the Sudan. The author endeavors to use not merely the scarce written sources, but also African oral traditions, linguistic evidence, and archaeological data. The chronological limits are approximately the fifth through the fourteenth century. The book comprises three parts of unequal length: Part I, "Introductory: The Byzantine Notitiae of the Egyptian Diocese," is brief and contains no new information. Part II, "Blemmyes, Novadae and the Christianization of Nubia," contains a number of original points. The author's main contribution here is an argument that "Blemmyes" and "Novadae" are loose, generic terms applied by classical writers to enemies of the Roman Empire on the Nubian frontier of Egypt. These terms were, therefore, not really the names of two specific ethnic and political groupings of Nubian peoples. But the author's contention that "Blemmyes" and "Novadae" were not used by these peoples to describe themselves is not so persuasive in the face of an inscription, which he even cites, made by a ruler who designated himself *"Basileiskos tōn Blemmyōn"* ("kinglet of the Blemmyes"). The author does perceive, correctly in my view, some differences between the Blemmyes and Novadae groups: the Blemmyes, unlike the Novadae, did not convert to Christianity, and each loose grouping dwelt in slightly different areas. In Part III, "The Central and Western Sudanese Sector and Sahara," the author boldly tries to trace Byzantine and Christian influences in central and western Sudan. This analysis is conjectural and lacks satisfactory documentation. One must emphasize that very few Byzantine literary references exist on any of these topics. Far more caution would seem warranted in employing oral traditions first recorded in the eighteenth, nineteenth, or twentieth centuries for any discussion of events in the remote sixth and seventh centuries. Furthermore, there is no discussion of the extent to which any of these supposed traces of Christianity may have non-Byzantine origins (that is, Abyssinia or

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Latin Christianity of western North Africa). There must be more epigraphic and archaeological research (the author seems unaware of W. H. C. Frend's excavations in Nubia) before one can reach any secure conclusions. The author himself admits that he is "far from having reached any definitive conclusions." He cites an interesting but very late and therefore questionable tradition on the flight of Persian soldiers (about 628–629) from Egypt into Nubia and central Sudan. His discussion of the Tuareg seems inconclusive, unpersuasive, and out of place.

While the work contains two useful appendixes, it lacks such other aids as maps, illustrations of archaeological objects, and a bibliography. In sum, the author has written an interesting book that attempts to correct previous scholarly neglect of the pre-Islamic period, but his scanty sources prevent him from reaching many positive conclusions.

University of Chicago WALTER EMIL KAEGI, JR.

A HISTORY OF THE VIKINGS. By *Gwyn Jones*. (New York: Oxford University Press. 1968. Pp. xvi, 504. $9.75.)

IN recent years a series of excellent studies on the Vikings has appeared by Brondsted, Oxenstierna, and Sawyer, which supplement older classics on the subject written by Musset, Olrich, and Kendrick. Each of the more recent books has had some special emphasis and some serious omissions. It is all the more remarkable, then, that this new book on the subject is authoritative, complete, fresh, and indispensable. The author's studies of the Norse in the Atlantic have already won him fame and the gratitude of his scholarly peers.

This volume traces the history of the Vikings at home and abroad from A.D. 700 to 1070, with a brief section dealing with Scandinavian activity in Greenland and North America down to the time of Columbus. Every kind of evidence—from archaeology, numismatics, Western, Arabic, and Byzantine sources, saga literature, and even climatology—is included and dealt with in masterly fashion. One finds here a full discussion of trade and commerce during this period, of Scandinavian religion, of art, of advances in naval construction, of scaldic literature and much else, all accompanied by wonderful illustrations. The skill with which the author threads his way through the evidence provided us by the *Eddas* and *Sagas* is a magnificent tribute to his good judgment and deep knowledge of this subject. Not everyone will agree with all of his conclusions in such matters, but even those who do not must feel that this volume will long be definitive in every sense of the word and that it supersedes all others, despite their merit.

In a work so balanced, so scholarly, so complete, and so welcome, there are a few minor points that I find inadequately treated, a fact that may enhance its worth—like a mole on the face of a beautiful woman. The author seems to feel that Trelleborg and Aggersborg were military camps built by Sven Forkbeard. He appears unaware of or unconvinced by the evidence, supplied by Sidney Cohen, that shows us that they were temple complexes, and he is inadequate in presenting the intricate problems concerning the seagoing Wends of the

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southern Baltic coast and their relations with their Scandinavian neighbors during the tenth and eleventh centuries. He ignores the work of Polish archaeologists in this field.

These are, however, very minor flaws in a study that all of us must welcome and that can serve as a model of how evidence from diverse disciplines can be combined to give us a proper picture of peoples in Western Europe during the early Middle Ages.

University of Texas, Austin ARCHIBALD R. LEWIS

LES INVASIONS NORMANDES EN BELGIQUE AU IXe SIÈCLE: LE PHÉNOMÈNE ET SA RÉPERCUSSION DANS L'HISTORIOGRAPHIE MÉDIÉVALE. By *Albert D'Haenens*. Preface by *Léopold Genicot*. [Université de Louvain, Recueil de travaux d'histoire et de philologie. Fourth Series, Number 38.] (Louvain: Bureaux du Recueil, Bibliothèque de l'Université, Publications Universitaires de Louvain; distrib. by Éditions Nauwelaerts, Louvain. 1967. Pp. vi, 391, 8 plates. 420 fr. B.)

EVER since the ninth century when the Frankish liturgy intoned, "From the fury of the Northmen, O Lord, deliver us," historians have asserted the disruptive effects of the Viking invasions on Western European society. P. H. Sawyer's more recent and careful investigations on England, however, have begun to minimize the total effect of the invasions. To offer a case study in this problem, Albert D'Haenens has focused on the region of modern Belgium during the ninth century. In the second half of his work he has exhaustively catalogued all of the extant historical documentation, paying special attention to chronology, affiliations, and reliability. Particularly surprising is the paucity of archaeological findings, which can be reduced to five spear points, three swords, some axes, and five bow carvings. Within Scandinavia no Frankish booty that cannot be explained by peaceful acquisition has been found. To this collection of sources the author has posed in the first part of his work a series of essential questions: Where, when, how, and why did the invaders penetrate the region? How did the victims resist, and where did they flee? What were the material and psychological effects on society? D'Haenens' conclusions for Belgium are remarkably close to Sawyer's for England. The Viking bands rarely exceeded a few hundred raiders. They burned and killed, but rarely had time to destroy permanent stone structures. Their goal was easily portable booty in gold, silver, and slaves. Their incursions produced little lasting damage on the region. Their chief victims were churchmen who fled with their treasures and relics.

This last factor is very important in interpreting the historical significance of the Viking invasions. Not only were ecclesiastics the principal object of the raids, but they produced the sole surviving accounts of the invasions. Without exception all written evidence concerning the Vikings comes from the pens of outraged churchmen. (With two exceptions none were direct witnesses of the events they report.) There is little doubt, therefore, that the Viking incursions profoundly shocked the clergy of the ninth century and the succeeding Middle Ages, but it is also clear that historians until recently have viewed the invasions essentially through clerical eyes. While the historians' dependence on clerical

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sources cannot be remedied, the critical and methodical researches of Sawyer and D'Haenens are a necessary corrective to this point of view.

Johns Hopkins University JOHN W. BALDWIN

DER HERZOGSTITEL IN FRANKREICH UND DEUTSCHLAND (9. BIS 12. JAHRHUNDERT). MIT LISTEN DER ÄLTESTEN DEUTSCHEN HERZOGSURKUNDEN. By *Walther Kienast.* (Munich: R. Oldenbourg Verlag. 1968. Pp. 505. DM 110.)

A FIVE-HUNDRED-PAGE book on the ducal title in medieval France and Germany would seem a classic example of too much spent on too little. And Kienast's major conclusions could surely have been presented in the form of a long article: the French dukes were not "tribal" (*Stammherzöge*) but rather "feudal" (*ducs féodaux*); with the exception of the dukes of Burgundy and of the short-lived *ducatus Franciae* (both recognized by the king) their titles were self-created, an expression of greatness rather than an indication of higher rank; in their titles ethnic and territorial qualifiers appear interchangeably and cannot be used to found any institutional hypotheses. In the case of this book, however, it is not the conclusions, or even the ostensible subject, that make the volume important. Rather, it is the ground the author traverses in getting from his original question ("Were the French dukes essentially like the German dukes?") to his answer ("No"). The problem, Kienast asserts, is first of all one of terminology. Who were styled "duke" by others, and especially by the kings? Who called themselves "duke"? Since no guide exists to answer that question, Kienast has put one together.

In his introduction he discusses, in turn, the geographical meaning of Francia in the tenth and eleventh centuries, the verbal division of the Western Kingdom into France, Burgundy, and Aquitaine, and the use of the terms *dux* and *ducatus* in Carolingian diplomas. He then surveys in successive chapters Francia, Burgundy, Normandy, Brittany, Aquitaine, Gascony, Toulouse-Narbonne, the use of the ducal title in German royal documents, and the use of the title in German ducal documents. Each chapter is composed essentially of a list of persons entitled "duke" in some official document and a tabulation of the varieties of form and the kinds of documents in which the title is found. Chronicles and other "unofficial" documents are systematically excluded. Together with the footnotes which list these documents in chronological order and discuss their authenticity, occasionally at great length, these form an invaluable guide to the published acts of all French and German ducal houses up to about 1200. Although this guide cannot be claimed to be complete, since Kienast has searched for unpublished documents only in a few departmental archives in southwestern France, the list of publications he has surveyed does include 1,062 titles. Kienast would have better served the user had he composed single lists for all of the French ducal houses as he has done for the German. But the inconvenience of searching his footnotes is minor compared to the labor he has saved all future historians studying the activities of the medieval dukes of France and Germany.

Amherst College FREDERIC L. CHEYETTE

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THE BISHOP-ELECT: A STUDY IN MEDIEVAL ECCLESIASTICAL OFFICE. By *Robert L. Benson.* (Princeton, N. J.: Princeton University Press. 1968. Pp. xix, 440. $11.50.)

To say that this is an excellent book is an understatement; it is the fruit of years of studying primary and secondary material, and the author deserves warm congratulations on an achievement that might well be the envy of writers nearing their retiring age. Within his chronological limits—the twelfth and thirteenth centuries—there is no work with which it can profitably be compared in scope, depth, and the perusal of sources. The subject itself is tricky, since many factors contributed to its full institutionalization. But the author is thoroughly familiar with both the canonistic sources, many of which are still unedited, and also the narrative, polemical, publicistic, and papal literature. The extent of the research is best indicated by the two-score manuscripts consulted and cited. Dr. Benson clearly delineates the juristic ingredients and the complexity of such legal entanglements as regalian rights, temporalities, and administrative powers that surrounded the juristic figure of the bishop-elect. He possessed, as it were, only one-half of the authority of a full bishop; the second half followed upon consecration and the conferment of the *potestas ordinis.*

The transition from the ascending to the descending theme of government and law is very well illustrated here by the excellent account of the doctrines of Stephen of Tournai. Despite some unavoidable technicalities, the reader is easily carried along. The author also indicates the necessity for further research in such areas as the concept of "canonical election" in the period after Gregory VII. Some weak points I have noted include the somewhat skimpy treatment of the principle of inalienability as far as it applied to the bishop-elect; the inadequate exposition and analysis of some Gregorian themes, such as the decree of the Lenten synod of 1080 (cap. 6) which begins the long development of the law of devolution; *Dictatus papae,* Number 23, might well have come within the purview of the author's cluster of problems. The intitulation of the bishops by the Grace of God and by that of the Apostolic See was two hundred years older than the author thinks; Innocent III's suggestive steps in introducing postulation and the instructive Huguccio passage might also have received a little more attention. But these are less than minor blemishes in what has proved to be a major contribution to legal history.

Trinity College, Cambridge WALTER ULLMANN

TWO CHURCHES: ENGLAND AND ITALY IN THE THIRTEENTH CENTURY. By *Robert Brentano.* (Princeton, N. J.: Princeton University Press. 1968. Pp. xvi, 372. $11.00.)

IN his preface Professor Brentano cautions the reader against taking his book at face value. Despite the sweep of the title, he does not intend to present a definitive exploration of the whole of the two churches, only "an extended essay about the connected differences between the two churches, to use 'aspects' as touchstones for comparison." The author warns the reader that many differences between the two churches arose from the fact that Italy was a country of city

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dwellers and England was not. He should also have pointed out that in the long-troubled history of medieval Italy, no century was more turbulent than the thirteenth and perhaps no century during the English Middle Ages was more serene.

Because so little is known about the thirteenth-century Italian church, the author concentrates on that country, even though the situation there would discourage a less ambitious scholar. The Italians lacked the English sense of efficiency as well as English reverence for legal records, and, of the documents they may have left, natural and man-made disasters have destroyed all but a few. In his search for evidence Brentano could find only crumbs, and what he regretfully admits concerning his study of Italian bishops, he could confess applied equally well to the other subjects he considers: the great majority of prelates "did not move enough in any direction to leave a decipherable trace in history."

Still the author feels able to offer some significant contrasts, even similarities. Italian dioceses were much more numerous than English, they were often poverty stricken, they suffered from fluid boundaries, and they were, in general, administratively unimportant. In their supervision, as was also the case with monasteries, the proximity of Rome invited constant interference. The bitter, time- and money-consuming controversies among bishops, monasteries, convents, and chapters over rights were, unfortunately, as scandalous in Italy as in England. The author declares that the bishops of Italy lacked the administrative ability of English prelates and were less conscious of their pastoral responsibilities. As testimony he offers the case of Bishop Sutton of Lincoln who left a codex and six rolls of register as against the single document of Manasses, bishop of Volturara. But one might ask whether that comparison is any more valid as evidence of the inferiority of Italian bishops than the contrast he presents between the energetic Winchelsey of Canterbury and the simple-minded Celestine V who would have remained a hermit however many tiaras they had piled on his head. Quite a different picture of Italian talent would have emerged had the author pitted Winchelsey against Innocent III or against either Gregory IX or Innocent IV, the other great Italian popes of that century.

Brentano uncovers much interesting material that should whet the appetite of many young scholars for years to come.

Pennsylvania State University JOSEPH H. DAHMUS

EDWARD I. By *L. F. Salzman.* (New York: Frederick A. Praeger. 1968. Pp. 224. $5.50.)

DR. Salzman, whose work has enriched English history for more than sixty years, offers his latest contribution in this biography of King Edward I. The book is designed to appeal to the general reader, and it probably will do so, for it is lucidly written. It is also good history.

Salzman's Edward I is proud and confident, conventionally pious, a good husband and father in both his marriages. He is an excellent administrator, but, above all, he dreams of war and empire. He is less careful of his subjects and less patient with adversaries than the Edward of Sir Maurice Powicke's *The Thirteenth Century,* and his government tends to autocracy. Salzman is not sure whether he

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ever really meant to return to the high mission of crusade during his reign. All in all, King Edward has never before been so well portrayed as in this book.

The author writes history on a narrow front. He is interested in the man Edward; he says only a little about the nation that Edward ruled or the ramifications of the affairs in which he was involved. This kind of history is unusual nowadays, but Salzman makes it refreshing and enlightening. Historians lost in the rolls and records may find it worthwhile to remember the King's concerns for his family, the court festivals, the sensational crimes that rocked London. Edward's itinerary becomes an unrolling ribbon of life. The King loved music, but cared little for such pursuits as books or architecture.

There are limitations in this approach. Salzman is not at home with law and governmental institutions, and his readers will learn nothing much about these matters. Writing of the organization of armies, he ignores compulsory knight service at the king's wages. It is extraordinary to read that the Hundred Years' War "would have been avoided" if Edward II had not married Isabel of France. The author is harder than he should be on Church and clergy. I do not know why it was a "strange infatuation" for Pope Boniface to believe that he could organize a crusade, as though previous popes had never done so. The author does not understand that married men could enjoy benefit of clergy, and he makes cruel sport of a letter in which Archbishop Pecham, asking mercy for Thomas of Weyland, revealed the whole uneasy opposition in the medieval mind between the rigor of justice and uncovenanted mercy.

Notwithstanding such limitations, it is an instructive book.

University of Iowa DONALD W. SUTHERLAND

THE RUSSIAN RELIGIOUS MIND. Volume II, THE MIDDLE AGES: THE THIRTEENTH TO THE FIFTEENTH CENTURIES. By *G. P. Fedotov*. Edited, with a foreword, by *John Meyendorff*. (Cambridge, Mass.: Harvard University Press. 1966. Pp. xiv, 423. $12.00.)

WHEN G. P. Fedotov died in 1951, he left unfinished a study of the religious life of Russia from the Mongol invasion to about 1550, which he intended as a sequel to his *The Russian Religious Mind: Kievan Christianity* (1946). John Meyendorff has given us a thorough and judicious edition of the intended study; it contains few factual errors, and textual gaps are partially filled from the author's earlier works. Mr. T. E. Bird has added a bibliography of Fedotov's writings. Both volumes are an expansion and revision of Fedotov's *Sviatie Drevnei Rusi:* X–XVII *St.* [The Saints of Ancient Russia: 10th–17th Century] (1931).

Fedotov's sequel on the religious life of Russia from the thirteenth to the fifteenth century is of uneven value. Most of the chapters, such as those on the chronicles, on Novgorod, on art and religion, and on Christian ethics are interpretive surveys of interest chiefly to the general reader who does not know Russian. A few, such as that on the heresy of the *Strigolniki,* are also valuable to the specialist. Fedotov was a careful and imaginative scholar; his analyses of religious texts still are largely valid despite the significant contributions that Soviet scholars have made to our knowledge of medieval Russia during the last

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fifteen years. Because Fedotov believed that saints produced the great achievements of religious culture, seven chapters (half of the book) deal with the careers and the hagiography of Russia's holy men. His best chapter is on the enigmatic and influential St. Sergius of Radonezh, founder of the monastic movement that spread across Great Russia from the late fourteenth century. Fedotov notes Sergius' patriotic call for unity against the Tatars as well as his ambivalent attitude toward the ambitions of Moscow. While accepting hagiographical accounts of the asceticism of saintly founders of monasteries, Fedotov is aware of the paradoxical acquisitiveness of the same monastic elite so well documented in the recent study by I. U. Budovnits, *Monastyri na Rusi.*

Most historians today have little sympathy for Fedotov's guiding belief that the "wholeness of religious personality is the invisible center out of which the main phenomena not only of religious but of cultural life in general have their origin and receive their meaning." On this premise Fedotov sees the Kiev and the St. Petersburg periods at times of creative freedom and the intervening Moscow period as a time of decadent slavery. Fedotov explains "The Tragedy of Russian Spirituality" in Moscow as the result of the victory of Joseph of Volok and his monastic movement, with its religion of fear, over Nilus Sorski and the monks of northern Russia, with their religion of love. Fedotov's comparison of the two saints is based on solid scholarship; his explanation of the rise of absolutism in Moscow is one dimensional and subjective. Also, Fedotov and other writers on medieval Russian culture have yet to explain satisfactorily the similarities in outlook and language of the Josephians and their opponents and the fact that Josephians, like Metropolitan Macarius who defended the worldly church, also canonized most of the saints of northern Russia and sponsored editions of their "Lives" that stress their contempt for worldly goods. Perhaps the unity of the cultural tradition that inspired contending clerics in the sixteenth century should be emphasized, rather than their now well-documented differences. If so, one must then expect to find many of the seeds of religious authoritarianism in Kievan Rus and must look at religious culture in a much broader context to understand why these seeds yielded such a rich harvest in Moscow.

Roosevelt University DAVID B. MILLER

A HISTORY OF THE FRANCISCAN ORDER: FROM ITS ORIGINS TO THE YEAR 1517. By *John Moorman.* (New York: Oxford University Press. 1968. Pp. xii, 641. $14.00.)

DR. John Moorman, bishop of Ripon, has combined religious faith, patient scholarship, and lucid writing in a lengthy, accurate chronicle of the Franciscan order's first three centuries. The earliest of these centuries deservedly occupies slightly more than one-half of the book. Students can turn with confidence to this work when checking such things as the official status of the order as outlined in a particular papal bull, or the material welfare of the order under a particular minister-general. The theme, as distinguished from the formal subject, is the persistent tension between the idealism of St. Francis and the behavior of the Franciscans.

Moorman's work derives from three main influences, the first of which is

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devotion to Francis. Though Francis has evoked sympathy from people of widely varied temperament and persuasion, the devotion expressed here is specifically that of the *fin-de-siècle* revival of Francis. Viewed in this way, the sensitive and sickly saint is a figure who brought renewal to the human soul, a warm appreciation to nature, and a personal, mystical perspective to religion. For this Anglican cleric, as much as for the Protestant Paul Sabatier or the nonbeliever Oscar Wilde, Francis is a saint for all men.

The second noteworthy influence can be seen in the author's scrupulous regard for factual evidence. Such regard, like sympathy for Francis, is not restricted to the late nineteenth century. And yet the methodology used here reflects the canons of that era's scientific history. Moorman's purpose in telling this story is not to praise God, or to teach by example, or to test some social scientific hypothesis; it is "to fill an obvious gap in an important field of medieval history." The story is laid out as simply and with as little speculative intrusion as possible. The facts, as it were, are there to speak for themselves.

The third influence to mark Moorman's study is a partial synthesis of the first two: an application of scientific history to the story of Francis and his followers. This combination is no more unique than either of the two original points separately, but Moorman's way of combining them reflects precisely the English school of Franciscan studies, an unofficial school whose unofficial dean was Andrew George Little. It was Little who first made the synthesis, by learning the new methodology at Dresden and Göttingen, and then by abandoning Domesday for the Friars Minor. The result was his study of the Franciscans at Oxford, published in 1892, a work that epitomizes what one used to mean by the term "definitive."

By staying close to these venerable influences, Moorman has written a solid if eclectic book. It is strong for being a labor of love and respect. The fierce controversies within the order are, moreover, presented with remarkably fair treatment all around. As for presenting the life of Francis and the early years of the order, probably no one is better qualified than Moorman, who worked his way through all the texts and textual controversies and in 1940 published his own study of the sources with the imprimatur of Little. Moorman's work is solid, finally, because so many aspects of the order's history are treated, including constitutional issues, intellectual accomplishments, ecclesiastical politics, and the development of the Poor Clares and the tertiaries.

There are weak points as well. The book provides no relevant insight into the workings, conscious or unconscious, of the mind of Francis. To call him a "man of genius" is to wander from textual evidence without gaining the benefits of modern scientific analysis. As indicated above, the book has a theme, and yet it lacks an argument. The book has no conclusion because there is no argument to complete or to sum up. Moorman includes a chapter on "The Schoolmen," but says nothing of Scholasticism. The fact that some Schoolmen happened to be friars or vice versa is assumed; the connection is neither explained nor even questioned. In discussing humanism, Moorman cites only Renaudet's work of 1916. The extensive and very stimulating work of recent years by continental scholars on such subjects as spirituality or poverty goes unnoticed. Instead Moorman cites E. S. Davison (1927) on the apostolic reform movements of the twelfth

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century—old material, indeed. In place of social analysis, or of any other explanation of the Franciscan phenomenon, there is only Moorman's lofty observation: "It was the 'common people' who heard Christ gladly; and it was to the common people that the friars directed their footsteps."

University of Chicago LESTER K. LITTLE

THE EMERGENCE OF MOSCOW, 1304–1359. By *J. L. I. Fennell.* (Berkeley and Los Angeles: University of California Press. 1968. Pp. 352. $9.50.)

THE over-all thesis of this political history is that the groundwork for the gradual strengthening of Muscovy and the development of its ability to resist the Golden Horde in the years after 1359 was laid in the period 1304–1359, a thesis that conforms with previous scholarly opinion, especially the views of Presniakov. Yet the explanation is, in part, novel. Disagreeing with previous scholars that the principality of Moscow had any advantage in its geographic location or its politics over its archcompetitor for Tatar favor, the principality of Tver, Fennell states that Tver suffered from certain disadvantages: it failed to win the support of the Orthodox Church; it encountered hostility from the great northern commercial republic of Novgorod; and it was so strong that the Tatars regarded it as less reliable than its weaker rival, Muscovy. The rise of Lithuania posed a threat to the Tatars, heightened by Lithuania's good relations with Tver, Pskov, and finally Novgorod, and, in the 1330's, obliged Khan Uzbek to abandon the traditional Tatar policy of playing one Russian prince against another and to adopt the new policy of allowing one principality (Moscow) "to grow strong in order to provide an effective barrier to Lithuanian expansion."

Khan Jani-Beg modified his father's policy, however, by refusing Semen of Moscow control over eastern Volga lands and even by displaying some favor to Nizhni-Novgorod and Riazan. Moscow, nonetheless, neutralized the Lithuanian threat by supporting Pskov's independence in 1348 and by adopting a policy of firmness and friendship with Novgorod. Although Semen's successor, Ivan II, foreshadowed future policy by his temporary hostility toward the Horde and friendliness to Lithuania and the West, from 1357 he again followed traditional policies favorable to Moscow's rise.

Fennell emphasizes that Tatar and Muscovite leaders often employed indirect and devious methods; it is logical, therefore, that he should seek carefully to reconstruct their plans and clarify their motives, at which he is very successful. Indeed, his greatest contribution may lie in the development of a thesis supported by Paszkiewicz, that the Tatar khans acted consciously in following a policy of siding with Muscovy against Lithuania in place of the traditional policy of playing off competing Suzdalian princes against one another. Fennell's attribution of a clear intention to the Tatar khans has as its corollary his unwillingness, rooted in a careful analysis of the chronicles, to attribute to Muscovite princes, boyars, and churchmen an intense desire to promote Russia's unity, as Presniakov and other scholars have done. Occasional deductions not adequately supported make the reader wonder whether Fennell may not view some matters with excessive clarity. For example, without offering a factual basis, Fennell assumes the existence of a pro-Western or pro-Lithuanian faction in Novgorod in 1330. On occasion he

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does not reveal his scholarly disagreements, as with Cherepnin's view that Pskov secured its independence in 1348 largely because Olgerd of Lithuania got no troops from Jani-Beg with which to oppose Pskov, not because Moscow helped Pskov, as Fennell maintains.

Clearly written in a pleasing style, this book contains such useful features as an index, a glossary, a discussion of sources, genealogical tables, and maps. It will long be read, but its ultimate place in the historical literature will depend in particular upon whether new documentary discoveries corroborate Fennell's views of the conscious and determinative role of the Tatar khans in Muscovite history.

University of Kansas OSWALD P. BACKUS III

ARCHBISHOP JOHN OF JENSTEIN (1348–1400): PAPALISM, HUMANISM AND REFORM IN PRE-HUSSITE PRAGUE. By *Ruben Ernest Weltsch*. [Studies in European History, Number 8.] (The Hague: Mouton. 1968. Pp. 252. 30 gls.)

IN this scholarly monograph, Ruben Weltsch has presented the third archbishop of Prague as the victim of a transitional age. Born two years after the accession of Charles IV to the throne of Bohemia, John of Jenstein had an easy start on his ecclesiastical career and was selected for a high position at the court of Charles's son and successor Wenceslaus IV. The Jensteins did not possess vast domains, but distinguished themselves in court service. Their close relative, John Očko of Vlašim, occupied the see of Prague from 1364 to 1380 and was the first Czech to receive the cardinal's hat.

Both the family background and long studies in French and Italian universities augured well for Jenstein's career. His biographer is, however, soon confronted with puzzling problems upon which the available sources are not very helpful. The combination of the highest church dignity in Bohemia with the chancellorship, from which seemingly nothing could have been expected but innumerable benefits, soon engendered troubles from which the gifted and ambitious cleric emerged deeply hurt and disillusioned.

The temptation to write a biography in which the dramatic turns in Jenstein's life would come to full view must have been strong, but Weltsch decided on topical rather than chronological organization and de-emphasized the emotional element stressed either by the archbishop himself in his autobiographical writings or by his opponents. Weltsch selected five subjects for a detailed treatment: the Great Schism and its effects on Church life in Bohemia; the clash between King Wenceslaus IV and the archbishop; Jenstein's religious personality; his learned controversies with Adalbertus Ranconis, the most distinguished scholar among contemporary Czechs; and, finally, the archbishop's relation with the champions of Church reform in the pre-Hussite era. A collection of passages from the archbishop's unprinted writings is a useful supplement to the five analytical chapters as it is unlikely that a critical edition of Jenstein's works will appear any time soon.

A book on John of Jenstein is a valuable addition to studies in English dealing with late medieval history of Bohemia, which have appeared in the past two decades. Its author, trained in medieval history by Professor S. H. Thomson, has brought together much interesting material that is, otherwise, not easily accessible

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to American scholars. Of the five chapters treating various facets of Jenstein's life, the third one is, in my opinion, the most successful. Although both Jenstein's writings and information emanating from other sources give insight into the depth of the archbishop's soul, a high dose of subtle perception was needed to facilitate the author's orientation in the labyrinth of contrasts and contradictions that puzzled both the contemporaries and modern historians. More sympathetic in his judgment of the archbishop's motivation than the late Václav Novotný, but not uncritical in evaluating the merits of conflicts into which the excitable Jenstein was drawn, the author has succeeded in presenting a balanced story that contributes substantially to our understanding of the age of transition in Bohemia from the blessed reign of Charles IV to the incubatory stage of the Hussite revolution.

University of Pennsylvania OTAKAR ODLOZILIK

THE BRUGES MONEY MARKET AROUND 1400. By *Raymond de Roover*. With a statistical supplement by *Hyman Sardy*. [Verhandelingen van de Koninklijke Vlaamse Academie voor Wetenschappen, Letteren en Schone Kunsten van België. Klasse der Letteren, Jaargang XXX, Number 63.] (Brussels: the Academie. 1968. Pp. 179.)

PROFESSOR de Roover and Mr. Sardy undertook an interesting experiment. Using a set of documents and letters left by a notable merchant of Prato, one Francesco di Marco Datini (1335–1410), they examined the whole money market in Western Europe around 1400. De Roover applied more traditional methods of historical interpretation of sources; Sardy used statistical methods, availing himself of a computer. Thus, the book attempts to analyze the money market through the use of two distinct and different methods. The results achieved by both scholars proved highly convergent, although Sardy's conclusions reveal some additional problems.

The data of both scholars are derived from the business letters of Florentine firms in Bruges—Giovanni Orlandini, Alberti, and Piero Benizzi & Co.—as well as Mannini in London between 1384 and 1410. After 1395 the authors had at their disposal a series of exchange quotations in five Western European banking centers. Second only to Bruges was Barcelona, where 1,000 of a total of 2,380 letters were sent by Orlandini.

For De Roover, this book continues and completes his earlier studies on banking and credit in medieval Bruges. As before, he argues that bills of exchange enabled bankers to avoid the canonical interdiction against lending at interest. Business grounded on bills of exchange could be looked upon as simple buying and selling of foreign currency, and theologians, although not deeply convinced, had to yield. Rate of profit was, of course, concealed, but an additional factor has to be considered: the risk connected with the changing course of exchange. Canon law permitted profit as a reward for risk. De Roover shows that until the eighteenth century the entire banking system adapted itself to these rules. Consequently, he argues that Max Weber's thesis about Catholicism as a deterrent in the development of capitalism is not valid. This reasoning appears too confining, but there is no room here for a broader discussion.

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De Roover opposes the view of R. Ehrenberg, who denied that the Bruges exchange formed an archetype for the subsequent Antwerp exchange. De Roover's argument seems to be right and suggests that already the Bruges money market displayed, in a lesser degree, many features peculiar to that of Antwerp.

Particularly interesting are Chapter III, "The Economic Determinants of Exchange Fluctuations," and Chapter IV, "The Chronicle of the Money Market." In Chapter III De Roover analyzes those factors influencing exchange rates in the Middle Ages, including the disguised profits of bankers, shortage or abundance of money on the market, changes in the value of coins, and the bearing of the balance of trade on the money market. Many of these topics are hard to explain because of the lack of sources, and De Roover does not always get far beyond the conclusions of his earlier works. Most important is his presentation of the policy of Burgundian rulers from 1390 to 1410, which aimed at maintaining or even improving money in Flanders. This deflationary policy only increased the difficulties of the slackening Flemish drapery industry. The author supports the policy of keeping the mint exclusively as an important source of income for the ducal treasury, but his argument does not seem entirely adequate. The nobility, the clergy, and other rent takers were highly interested in this policy, and those groups formed the social basis of the Burgundian state.

The bankers endured severe hardships because of monetary reforms. They were limited in the use of bills of exchange, and one of the reforms even tried to force upon them cash operations in depreciated coins. The situation was aggravated by artificial depreciation of gold coins in favor of silver ones.

A most interesting point is the influence of the goods trade on the money market. It is chiefly a problem of abundance (*larghezza*) or shortage (*stretezza*) of money, which has been revealed through the commercial and banking manuals of the time. De Roover proves that the mass purchase and sale of goods while Italian galleys were in Bruges depended on the arrival of Hanseatic merchants, who influenced the money market and the prices of bills of exchange. The money market in Bruges, Barcelona, and Venice experienced yearly seasonal fluctuations that were later so characteristic of sixteenth-century Antwerp. De Roover denigrates state intervention in periods of money shortage; he thinks it to be more a factor of anarchy than of order. At the turn of the fourteenth century the tasks of the bankers were, therefore, rather complex. They were able to anticipate market development for only a few weeks, and too many factors were remote and escaped their attention. Because travel was slow, contact between banking centers was difficult.

Chapter IV is more descriptive. In dealing with the money market in Bruges between 1395 and 1410, it weighs various factors of change. It constitutes a kind of inductive plea for the above theses, and it is closely based on source material. De Roover believes that the money market in Bruges began to form some time after 1300, when the Champagne fairs were diminishing in importance. By 1345 it was already established, and it continued through the fifteenth century. In the sixteenth century it was transferred to Antwerp. The history of that big center of economic life requires further investigation. The author urges maximum caution, however, in such research.

Sardy, having used the same source material, correlates exchange rates between Bruges and Barcelona, Genoa, London, Paris, and Venice, as well as

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between Barcelona and Avignon, Bruges, Florence, Genoa, Majorca, Montpellier, and Pisa from 1395 to 1410. I am no statistician and am not familiar with the methods applied; they seem interesting and legitimate. Sardy, too, concludes that medieval banking centers witnessed money fluctuations, but in general they proved strongly chaotic. The results of statistical investigations do not always correspond with the data of merchant manuals.

An important issue arises in attempting to establish correlation of exchange rates in particular centers of the money trade; that interaction did not, however, embrace the whole of Western Europe. This is no surprise for a medievalist, but it provides one more proof of the weakness of the international economy in the late Middle Ages.

Sardy stresses the fact that bankers and exchange dealers understood such phenomena as *larghezza* and *strettezza* and tried to profit from them. International trade cannot always, however, explain money fluctuations and the exchange rate of bills. About one-third of the fluctuations visible in the discussed series of figures can be ascribed to the impact of rate of profit disguised in the price of foreign exchange, which resulted from canonical legislation against usury.

Both papers tackle interesting problems. Although they do not fundamentally change the prevalent view of the late medieval economy, they make it more concrete and positive.

University of Warsaw MARIAN MAŁOWIST

GESCHICHTE THÜRINGENS. Volume III, DAS ZEITALTER DES HUMANISMUS UND DER REFORMATION. Edited by *Hans Patze* and *Walter Schlesinger*. [Mitteldeutsche Forschungen, Number 48, Volume III.] (Köln Graz: Böhlau Verlag. 1967. Pp. 371. DM 48.)

REGIONAL history, if well written, can be valuable and interesting for the general historian, and such is the case here. The text contains two rather lengthy chapters: one dealing with humanism and the Reformation, the other with the political history of Thuringia from the division of the Wettin lands between the Ernestine and Albertine lines of the house in 1485 until the partition of the remaining Ernestine Thuringian holdings in 1572.

The first chapter is by Professor Irmgard Höss, author of a highly regarded study of Georg Spalatin (1956). Such men as Spalatin and other members of the Mutian circle of humanists at Erfurt at first labored to introduce the New Learning and then, in many cases, emerged to help guide to a successful conclusion the Reformation movement initiated by Luther at Wittenberg.

Although Thuringia proved to be receptive also to the teachings of Karlstadt, Münzer, and, later, the Anabaptists, it was the Lutheran doctrine that gained the support of the predominant Ernestine rulers. Much of the interest of the first chapter of this work lies in the detailed picture it presents of the ways in which the electors and their officials actually effected the consolidation of the territorial Reformation: the visitations, the establishment of the office of church superintendent, the unsatisfactory results of the one attempt to install a Lutheran bishop (Naumburg), the sequestering and ultimate confiscation of monastic property.

The chapter on political history, by Thomas Klein, clearly reveals that the administrative centralization characteristic of the times was carried out in Ernes-

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tine Thuringia against the backdrop of an increasingly precarious position in relation to the Empire and neighboring German states. Much of the study is devoted to an analysis of the imperial and foreign policies of the Ernestine rulers, which, from the 1540's on, led to repeated disasters. The result was the loss of the Ernestine base in electoral Saxony as a consequence of the Schmalkaldic War and, ultimately, the triumph of *Kleinstaaterei* in a fragmented Thuringia.

The scholarly value of this useful volume is enhanced by the inclusion of almost forty pages of footnotes that contain some references to primary source materials and an especially helpful, up-to-date survey of the more important secondary works. The book, in addition, contains many well-chosen illustrations, several good maps, and a very detailed index.

University of Colorado CARL C. CHRISTENSEN

Modern Europe

ART, SCIENCE, AND HISTORY IN THE RENAISSANCE. Edited by *Charles S. Singleton*. [The Johns Hopkins Humanities Seminars.] (Baltimore: Johns Hopkins Press. 1967. Pp. vii, 446. $12.50.)

THIS is a most useful collection of essays on Renaissance art and thought, though its aims and accomplishments are decidedly uneven. Some of the contributions are invaluable because they include clear and wide-ranging summaries of recent research; others, despite a narrower focus, still suggest broad implications; the remainder, being highly specialized, technical pieces, are of limited interest.

E. H. Gombrich opens the volume with a stimulating analysis of the impact of criticism on Renaissance art. This is followed by an article by John White, which explores the relations between sculpture and painting. Though the article is often detailed, its scope is enormous, covering the thirteenth to the fifteenth century. The closing section, on the significance of Pollaiuolo, is masterly. Two essays on music conclude this first section of the book. Edward Lowinsky lucidly surveys recent research, mainly his own, into the advances accomplished during the Renaissance. Much monographic literature is thus made easily accessible to the nonmusicologist, though the historian might well balk at an explanation of developments in the Netherlands that portrays the Dutch as rebelling for freedom of conscience in 1568, particularly since the composers in question were active earlier in the century. The next article, Walter Rubsamen's investigation of Lorenzo de' Medici's carnival songs, will concern only the specialist.

The longest section of the book is devoted to the history of science. Frances Yates gives a fascinating account of the influence of the Hermetic tradition, which, again, provides a most useful summary of recent work that has brought about a reinterpretation of Renaissance intellectual history. Stillman Drake's contribution is to establish precisely and clearly why Galileo deserves a central place in the Scientific Revolution. Similarly, D. W. Waters' survey of developments in navigation during the fifteenth and sixteenth centuries will provide students with a lucid introduction to some striking accomplishments of Renaissance Europe, but one should beware of his excessive claims for his subject. Civilization is no more "the product of the activities of seamen" than it is

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the product of the activities of ink manufacturers, and Waters seems to have no understanding of the difference between science and technology. He suggests that the Scientific Revolution was begun, though "unwittingly," by Henry the Navigator and that measurement and observation were the basic elements of the revolution. Before venturing to make such sweeping assertions, it might have been wise for him to have pondered the rather different definitions of science implicit in the papers of Yates and Drake, not to mention those of Boas, Clagett, and McMullin.

George Boas analyzes the Florentine Neoplatonists, arguing that they "were the last philosophers to attempt the wedding of religion and science." Marshall Clagett's paper, a technical discussion of scientific advances in the fourteenth century, succeeds in defining the period as a distinct stage between medieval and sixteenth-century investigations. And Ernan McMullin suggests that the revolution that took place between Copernicus and Newton can best be understood by asking what the scientists thought they were proving: Did their findings correspond to reality? What did they mean by "hypothesis"? Was unwavering truth attainable? Like the paper by Yates, these are important contributions to the history of thought, but the historian must wonder whether ideas have quite as much a life of their own as these scholars suggest. For all his faults, Waters does relate the advance of knowledge to the outside world, and Drake is particularly persuasive because he is concerned both with Galileo's insights and with the question of why the great scientist was so effective a figure in his day.

The last section of the book, devoted to "History," starts with an all too brief reinterpretation by Felix Gilbert of the place of history in Renaissance thought. He argues that the subject was considered to have minor value per se, but that significant new ideas appeared nonetheless, notably in the assessment of documents. J. H. Hexter, in a brilliant comparison of Machiavelli, More, and Seyssel, isolates three streams in the history of political thought and suggests why each man evaluated his own times differently. How he comes to clarify the *Prince* by elucidating "the difference between the shark and Mac the Knife" must be left for the reader to discover. The final paper, by Wolfgang Clemen, is the only one to treat literature. It provides a suggestive examination of the change wrought by John Donne in the writing of poetry.

In sum, this is a volume that historians of European thought between the fourteenth and seventeenth century will find enlightening at various levels. There are useful syntheses of current research (notably the articles by Lowinsky, Yates, Clagett, Drake, McMullin, and, to a lesser degree, Waters); some highly stimulating and provocative theses (particularly in the articles by Gombrich, Boas, Gilbert, Hexter, and, to a lesser degree, White); and there are a few technical scholarly contributions. Each writer cultivates his own garden, but in some ways a clearer definition of the "Renaissance" emerges from these individual efforts than might have appeared had they all solemnly joined in a united effort to define the term. A sense of the period is imparted that generalities often fail to convey. For taking this approach, we must be grateful to Charles Singleton, who edited the essays and organized the seminar that produced them. Only two cavils can be raised: the illustrations might have been of better quality, and a somewhat closer uniformity of intention might have been demanded of

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the authors. Had they all aimed at a similar level (and had they all revised their lectures for publication with similar expansiveness), this volume would have been even more valuable than it is.

Princeton University THEODORE K. RABB

THE OTTOMAN IMPACT ON EUROPE. By *Paul Coles*. [History of European Civilization Library.] ([New York:] Harcourt, Brace and World. 1968. Pp. 216. $5.95.)

ONE of the latest additions to the studies describing the rise of Western civilization is Professor Coles's history of the Ottoman Empire in Europe. In this short work the author gives a five-part coverage of the history of the Ottomans, and their contact with Europe, between the fourteenth and seventeenth century. Without doubt this is a much-needed attack upon an area that has remained obscure for too long. Yet, once the reproductions of Turkish miniatures are passed, the content of this work has much more in common with Christian polemics of the Middle Ages than it does with the scholarly histories of the twentieth century.

Resurrecting the old European explanation for the success of Islam, the author associates the establishment of the Ottoman state with most violent measures: "the predatory operations of a nomadic war band," the harnessing of the "enthusiastic plunder expectations of an oriental horde," and the "massive enslavement of peasant populations drawn from the fringes of the territory under Ottoman control." The more peaceful reconstruction of a premodern government whose economic base rested upon the organization of agriculture and commerce in the Near East is not developed.

The contrast between the social and political structures of the Muslim East and Christian Europe has long been a source of interest to European writers. Thus the absolute power of the Sultan, the dominance of government by converted slaves, and the weakness of Ottoman family ties is the Turkish version of these old concerns. The difference between the two cultures is then summed up in the disparity between the historical success of European aristocracy and the failure of the equivalent class in the Ottoman Empire—the Muslim-born members of the ruling class—to advance beyond their status at birth. But all these erroneous ideas, representing an old European view of Ottoman society, have been thoroughly criticized by Professors Inalcık and Itzkowitz on the basis of original sources.

In the seventeenth century Christian writers compared the Wars of Religion in Europe with the struggles in the East between the heterodox Safawids and the orthodox Ottomans. Coles repeats this dubious comparison and implies that the Ottoman suppression of heterodox sects during the course of the Persian wars encouraged intellectual and moral abdication in the Empire through a rigidification of "the fanatical and intolerant religion of Islam." But during the Persian wars the Ottomans experienced a period of intellectual and artistic development and accepted non-Muslim exiles from the Iberian Peninsula.

When decline comes under observation, the author subjects Ottoman society to a series of criticisms based upon the nationalist arguments of Ziya Gökalp

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and the moral superiority of European society. As the Ottomans turned into an age of conservatism, soldiers transformed themselves into "lazy landlords," statesmen rose to the top in a "morally unscrupulous tradition," and reform programs were "pathetic" in comparison with similar European literature. All this simply raises the question of how the Empire survived under such deplorable conditions.

Finally, what is remarkable about this history is not its catalogue of errors but the vehemence with which the Ottoman Empire is condemned. The general reader will not, therefore, find Ottoman history unraveled on its own terms; rather, he will encounter an uncritical, twentieth-century version of an old dispute.

Temple University ANDREW C. HESS

ENGLISCH-ÖSTERREICHISCHE WIRTSCHAFTSBEZIEHUNGEN IN DER ERSTEN HÄLFTE DES 19. JAHRHUNDERTS. By *Hedwig Pavelka*. [Wiener Forschungen zur Wirtschafts- und Sozialgeschichte, Number 2.] (Vienna: Hermann Böhlaus Nachf. 1968. Pp. 192. Sch. 198.)

MODERN European economic historians will acclaim this scholarly and objective analysis of Anglo-Austrian shipping and trading relationships in the first half of the last century as a courageous undertaking. The pertinent sources—consular reports, records of harbor customs offices and chambers of commerce, Anglo-Austrian navigation and trade treaties of 1829 and 1838, and official Austrian and English trade statistics, for example—were widely dispersed, inadequate, and rarely comparable; nor do they document the extensive legitimate trade that did not enter the Adriatic or the imponderable but probably extensive and duty-free contraband traffic between the Adriatic ports and the Austrian hinterland. The author traces the reorientation of Anglo-Austrian trade from the Netherlands to Southeastern Europe, which was a result of the Treaty of Campo Formio and the ensuing continental blockade. Mutual Austrian and British anxiety over the formation of trade blocs such as the *Zollverein* is stressed, as are Britain's efforts to strengthen Austria as a rival of Russia in Southeastern Europe and as a potential political ally of England and Turkey. The roles of the British Board of Trade and of British consuls and vice-consuls in the Adriatic ports in fostering British exports of colonial and manufactured wares and in securing Austrian grain and timber for the United Kingdom are explored. The author contrasts Austria's agrarian economy with the growing industrial might of the British and shows how these divergent interests are reflected in the long persistence of Austria's prohibitive customs policy and the indifference and even hostility of the Austrian bureaucracy toward the expansion of foreign trade and the increasingly laissez-faire proclivities of the British. There is an enlightening discussion of Anglo-Austrian preoccupation with Danubian navigation, their mutual contacts in Malta and Gibraltar, the problem of diverse harbor and quarantine regulations in the Mediterranean, the Adriatic, and the British Isles, and the impact of recurring famine and disease on trade fluctuations. It is curious that the persistent efforts of British diplomats and consuls to allay Austrian official suspicion and the hostility of the self-serving monopoly enterprises fostered by the country's highly restrictive tariff, which was in force until 1851, did not lead to a significant expansion of Anglo-Austrian trade.

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This work is the product of painstaking research and reveals an excellent acquaintance with and use of the available sources; it will be valued, above all, for its author's insight into the dynamics of tariff and shipping regulation and the interaction of broad national economic and political interests and policies in both states.

Pennsylvania State University ALFRED G. PUNDT

INDUSTRY BEFORE THE INDUSTRIAL REVOLUTION: INCORPORATING A STUDY OF THE CHARTERED COMPANIES OF THE SOCIETY OF MINES ROYAL AND OF MINERAL AND BATTERY WORKS. In two volumes. By *William Rees*. (Cardiff: University of Wales Press. 1968. Pp. xv, 20–363; viii, 368–698. 126*s*. the set.)

THIS is a study of mining and metallurgy in the western half of England and in Ireland during the sixteenth and seventeenth centuries. As such, it is somewhat more restricted in content than its title would suggest. Yet, within its self-imposed limits, Professor Rees's work is, unquestionably, a major contribution to the knowledge of the early stages of industrialization and contributes much that is new in an important field. Building in part on the foundations laid earlier by J. U. Nef and H. R. Schubert, the author has marshaled an astonishing amount of new evidence to illuminate further not only the history of coal and iron mining, but also that of nonferrous metals and of metallurgy in general as it developed in Ireland, Wales, and the bordering areas. Part One deals mainly with coal mining; Part Two, with the mining and processing of metals; Part Three is devoted to a detailed account of the history of the two great chartered companies of Mines Royal and Mineral and Battery Works.

The two volumes will appeal primarily to the student of economic history and technology and will prove invaluable for an understanding of the slow, laborious process of technological experimentation and innovation; the problem of the choice of techniques, in particular, is thoroughly investigated. Nonspecialists may well be deterred by a text that bristles with technical details and immense erudition, but there is much to interest them as well. Rees's study of the origins, growth, and tribulations of two great chartered companies, for instance, tells much of the social and political context in which they were set, as well as of the changing assumptions and goals of government. What he has to say about the contributions made to virtually every aspect of mining and metallurgy by foreign, notably German, craftsmen and merchants greatly adds to our knowledge of the mobility of skills, talents, and financial resources in the early modern period. One more example, his account of the formation of a large mining enterprise in Wales under the leadership of Sir Humphrey Mackworth, represents a brilliant case study in entrepreneurial history vividly portraying the men involved as they confronted day-to-day problems of technology, management, and labor relations.

The book contains a generous selection of original documents and of excellent illustrations that will prove especially interesting for the student of early modern technology.

University of Wisconsin DOMENICO SELLA

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THE AUTHOR'S EMPTY PURSE AND THE RISE OF THE LITERARY AGENT. By *James Hepburn*. (New York: Oxford University Press. 1968. Pp. 135. $3.50.)

THIS slim volume helps to fill a large gap in the history of the English book trade. Much has been written hitherto about the development of authorship, printing, and publishing, but the rise of the literary agent has scarcely been chronicled. Thanks to Hepburn, we now have a coherent account of how these paid middlemen came to intrude themselves between authors and publishers during the latter decades of the nineteenth century.

The conditions of the seventeenth and eighteenth centuries that eventually give rise to the literary agent are discussed in the opening chapters of the book. By the nineteenth century more and more authors sought to make a living from their writings and to question whether they received their proper share of the publisher's profits. Friends, vanity publishers, and newspaper agents tried to mediate. Gradually the paid literary agent emerged, but not without stirring up resentment. Publishers objected to being played off against one another, and they did their best to steer authors away from these go-betweens. Even the Society of Authors, while no friend of the publishers, took a dim view of most literary agents.

Despite all obstacles the number of London agents continued to grow from a mere handful in the 1890's to about seventy today. By 1926 it was estimated that nearly 50 per cent of the novels, biographies, and other trade publications, as distinct from textbooks, were being handled by agents; this proportion has risen to 75 per cent in recent years.

Much of Hepburn's book is appropriately taken up with the real and imagined grievances of authors and the agents' response to these discontents. One of the most interesting sections has to do with the writing career of Arnold Bennett and his agent, J. B. Pinker. Hepburn, having previously edited a volume of Bennett-Pinker correspondence, gives us abundant evidence to show how effectively one of the leading London agents served his client. It is unfortunate that there is a comparative lack of similarly detailed accounts of other authors and their agents.

It is startling to learn that nowadays five agencies control about 90 per cent of the business, with Curtis Brown, numbering some fifty employees, leading the pack. Hepburn reveals why so few firms dominate the field: success breeds success, and a handsome stable of authors gives an agency more bargaining power with publishers.

The author briefly describes literary agents in the United States, but he does not emphasize sufficiently their significant role in Anglo-American publishing relations. Here Hepburn might have found a most important precedent. As early as the 1830's and 1840's literary agents were being employed by American authors to secure publication and a valid copyright in Great Britain. Colonel Thomas Aspinwall, consul general of the American legation in London, acted as a paid representative for authors like Washington Irving and William H. Prescott when it came to dealing with London publishers. Americans resident in London, such as the bookseller, Obadiah Rich, and the banker, Russell Sturgis, performed similar duties.

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The book concludes with a most valuable bibliography of twenty-three pages; it is regrettable, however, that the author rarely uses footnotes and forces the reader to rummage through the bibliography for references to specific points.

Wabash College JAMES J. BARNES

THE FIRST EARL OF SHAFTESBURY. By *K. H. D. Haley*. (New York: Oxford University Press. 1968. Pp. xii, 767. $17.75.)

"There appears to be insufficient material for a convincing life," so Professor J. R. Jones remarked in 1961 (*The First Whigs*), in dismissing previous biographies of the first Earl of Shaftesbury. Yet now we are asked to place beside the studies of W. D. Christie (1871) and L. F. Brown (1933) still another weighty attempt to make sense and significance out of Shaftesbury's seemingly inconsistent, disreputable, and unsuccessful career. How well, then, does Professor Haley overcome the relative paucity of extant Shaftesbury Papers? And how far does he succeed in convincing where previous scholars have faltered?

To begin with, there can be little doubt of the thoroughness of Haley's research. His quest for evidence has extended as far as the dispatches of the Spanish envoys in London (now in the Brussels archives), and he has even managed to turn up a few additional Shaftesbury items in the muniment room at St. Giles. Indeed, it was at St. Giles that he made, perhaps, his most valuable find—two of Shaftesbury's working lists (to be published shortly), which categorize all the peers and members of the Commons in May 1677 in various degrees of "vileness" or "worthiness." On this firm base, then, Haley is able to challenge and often to correct the work of his predecessors not only on specific points of detail, as, for example, the authorship of the proposed revision of the franchise and redistribution of parliamentary seats published posthumously under Shaftesbury's name, but also on some major points of interpretation. Most notable is his showing that the Earl was never the unquestioned head of a united opposition, either in the last years of the Cavalier Parliament or even in the heat of the Exclusion controversy, though he also argues that "the prestige of his name, the inspiration of his leadership, the impetus of his immense organizing ability, and the assistance of his tactical skill" were crucial factors in intensifying and prolonging the crisis of 1678–1683. There are, nonetheless, also times when the evidence, however skillfully culled, will not yield any clear conclusions, and on points such as Shaftesbury's relationship with Oates, Haley carefully avoids the posture of rhetorical precision.

But what of the author's principal aim: to pass judgment on the charges leveled against Shaftesbury by both contemporaries and later historians such as Macaulay of insincerity of purpose and unscrupulousness of method during his long career? Here, Haley takes his stand with Christie and Brown in defense of Shaftesbury, though by no means does his view of the Earl and his times always coincide with theirs. On the one hand, he labors to demonstrate that a certain consistency can be discerned in Shaftesbury's seemingly frequent shift of sides, especially from Royalist to Parliamentarian in the 1640's, from membership in the Barebones Parliament and the Cromwellian Council of State to opposition to the Protectorate and then to royalism, and from ministerial office

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under Charles II to opposition to him. Not only does he show that all but one of these changes of position brought no immediate material advantage to the Earl, but he also suggests that these twists and turns can best be understood as the choices of a believer in parliamentary government and religious liberty acting in logical fashion to further these ideals amidst the tumult of English politics between the 1640's and 1680's. On the other hand, Haley reminds us that, though Shaftesbury was certainly not above corruption, intimidation, and demagoguery, he was neither uniquely responsible for the extravagances of the Popish Plot nor any more uninhibited in his tactics than Charles II himself. To be sure, he admits that Shaftesbury's conduct cannot be altogether extenuated, but he does make a case that the constitutional, religious, and colonial-economic policies that the Earl persistently advocated were rather more enlightened than those favored by most of his contemporaries. And, finally, he concludes that though Shaftesbury died a frustrated politician-in-exile, his fears of a popish successor and his support for Exclusion were subsequently endorsed by the events of 1685–1689.

On all these major questions of interpretation the author's evidence and arguments induce assent, and not only by dint of repetition. There were times, nevertheless, when I felt that perhaps the defense brief was being pressed a little too far, as, for example, in the attempts to explain away Shaftesbury's backing for toleration by royal indulgence in 1663 or to explicate his famous *Delenda est Carthago* speech a decade later. I cannot, moreover, escape the suspicion that this book would be more usable had it been pared down in size. Whatever the merits of such reservations, Haley has clearly succeeded in completing that rehabilitation of Shaftesbury begun by Christie nearly a century ago, while at the same time producing what will certainly be the definitive biography for many years to come.

University of Iowa HENRY HORWITZ

SIR RALPH HOPTON: THE KING'S MAN IN THE WEST (1642–1652). A STUDY IN CHARACTER AND COMMAND. By *F. T. R. Edgar.* (New York: Oxford University Press. 1968. Pp. xvii, 248. $7.75.)

THAT Charles I was a poor judge of men, devoid of loyalty to those who served him best, will be no news to students of the English Civil War. Hopton, the subject of this admirable study, was probably the King's ablest general after Rupert, but his good sense and outstanding administrative powers were given full scope only when he led the Cornish army in 1643. In the previous September Hopton had only a handful of men left over from Hertford's debacle in Somerset, but on this slim foundation he built the army that rescued the west for Charles I. The Cornishmen gave him talented officers, rugged soldiers, high morale; Hopton gave them organization, discipline, and the leadership that carried them to Lansdown, Roundway, and Bristol. But, as Edgar shows, Hopton was essentially a country gentleman without court influence, and so the aftermath was subordination to a deaf and gouty old Scottish earl, and a pathetic anticlimax at Alton and Cheriton. Edgar provides a full, clear, and well-balanced survey of Hopton's campaigns and the organization of his armies. He has Hopton's own narrative,

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Bellum Civile, as a starting point, but assiduous combing of the primary sources enables him to construct a far better-rounded portrait of the Royalist commander than exists in the hitherto nearest thing to one, Firth's brief article in the *DNB.*

On military affairs Edgar's touch is sure; on politics, it is less so. This is a military, not a political, biography, but still the weakness is important because Hopton's failure to push himself forward after Bristol was in part the product of his political moderation. The Puritan who denounced church-ales, the reformer who voted for Strafford's attainder and led the deputation presenting the Grand Remonstrance to Charles I, was no Cavalier firebrand and might so easily have joined his friend Waller on the other side. It is unfortunate that Edgar seems unable to make up his mind at crucial points in his narrative. He is sensible in stressing Hopton's nonpartisan moderation and his attachment to the central "country" position, but he is not convincing on the reason for Hopton's shift between the Remonstrance and the attempt on the Five Members a month later. And Hopton, who shared the common preference for limited episcopacy, cannot have been both a "puritan Cavalier" and a "high Anglican."

With this exception, Edgar's book is a commendable contribution to Civil War history. "Pay well, command well, and hang well" was Hopton's triple recipe for generalship. Given the Royalists' chronic financial problem, the first was not always in his power, but, as Edgar shows, he conscientiously followed the other two.

Brown University DAVID UNDERDOWN

BOLINGBROKE AND HIS CIRCLE: THE POLITICS OF NOSTALGIA IN THE AGE OF WALPOLE. By *Isaac Kramnick*. [Harvard Political Studies, published under the direction of the Department of Government in Harvard University.] (Cambridge, Mass.: Harvard University Press. 1968. Pp. xiii, 321. $6.95.)

IN this scholarly addition to the growing body of literature concerned with early eighteenth-century political ideology, Mr. Kramnick's main concern is with Bolingbroke's political writings, and in four chapters that form the heart of his book he provides an important analysis of them. By placing Bolingbroke's works in a broad social and economic framework rather than in the context of his wayward and erratic career, he uncovers in them a coherent and consistent pattern previously unrecognized. He finds in Bolingbroke's writings a persistent concern with the changing balance between landed and other forms of wealth, and in particular with the threat to the traditional social and political order posed by the development of powerful financial institutions after 1689. Bolingbroke's writings throughout his career were informed by the same general concerns—the loosening of the traditional social bonds and the destruction of the simple, virtuous, and hierarchical society by the corrosive power of money. He may be regarded, therefore, as a spokesman for that significant and bitter section of landed society, especially the smaller gentry, who found themselves in economic difficulties in the first half of the century and identified their

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enemy as an alliance of the government, particularly Walpole's government, with the sinister world of finance and stockjobbing.

Such a slight sketch cannot do justice to Kramnick's skillful exposition of Bolingbroke's political and social ideas. These four chapters are rich in detail and are convincingly argued and uniformly interesting. To them Kramnick has added others on the reactions of other writers and politicians to the social and economic developments of the period, including a chapter on Pope, Swift, Gay, and Lyttelton—the "Circle" of the title—and, more surprisingly, on Walpole, on Defoe and Mandeville, and on a selection of "Commonwealthmen." These are not, on the whole, as well conceived or as carefully argued as the chapters on Bolingbroke. Walpole and Defoe seem to be in the book simply to provide contrast to the views of Bolingbroke; as supporters of the new economic order they act as foils for his ideas, illustrating the target and giving it substance. There is no doubt some general truth in the contrast, but Kramnick presents it as an unnecessarily exaggerated antithesis of "whigs" accepting and welcoming economic and social change and "tories" opposing and deploring it. His main points would not have been weakened if he had acknowledged the complexity of such attitudes or if he had recognized that neither Walpole nor his opponents were exclusively concerned about luxury as a factor in English life or the economic condition of the gentry. One might cavil too at the author's claim that the opposition in the 1730's was "held together by a particular set of ideas," if only because in the kind of book he has written he cannot provide the rigorous political analysis required to substantiate it. Many other points in the book will not win universal acceptance, but they are largely peripheral to Kramnick's central concern and to his real achievement: an illuminating analysis of Bolingbroke's political writings, which is a contribution of the first importance to eighteenth-century studies.

University of Toronto J. M. Beattie

SCOTLAND: 1689 TO THE PRESENT. By *William Ferguson*. [The Edinburgh History of Scotland, Volume IV.] (New York: Frederick A. Praeger. 1968. Pp. ix, 464. $14.00.)

Although undoubtedly the author did not intend to do so, this book raises the question as to whether, except as part of Great Britain, Scotland has had a history since union. Despite great efforts to demonstrate a "Scottish" phase of a larger British history, Professor Ferguson instead shows "the greater drawing the smaller," and Scots continuing to take the "Great Road" south to larger opportunities. The author deprecates the countermovement of English influences into Scottish law, education, and industry and appears no more able than Jacobites of the eighteenth century, the League for the Vindication of Scottish Rights in the mid-nineteenth century, or the Scottish Nationalist party in the twentieth to grasp the significance of the depth of Scottish penetration into British history. When such a history includes Scots as First Lords of the Admiralty, a vigorous Home Secretary, a strong Foreign Secretary, no fewer than seven Prime Ministers, and innumerable generals, admirals, and governors-

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general, such a history is something more than that of England writ large since 1707.

That English influences have made Scottish institutions less "pure" since 1707 does not imply, as the author somehow does, that they have suffered denigration. It is interesting that Scottish resistance to Anglicizing has been strongest in religion, a subject that the book handles well, particularly in its analysis of the Disruption in the Church of Scotland over the issue of patronage. Ferguson has also provided one of the best descriptions I have seen of the eighteenth-century Scottish political arrangements that permitted the Pitt-Braxfield repression of the 1790's. He has produced a full analysis of the Scottish Reform Act of 1832, usually ignored by writers in their concentration upon the English Reform Act. A similar concentration by contemporaries, Ferguson demonstrates, allowed Scottish reformers to gain considerably more than English Radicals, who had to work with Whig magnates. The author also refuses to bow to those traditional Scottish idols of education and regimental history, finding the one faulty at all times and the other exaggerated, and he points grimly to the poor Scottish record in health and housing.

There is, nevertheless, a defensive edginess throughout the book about the presumed "secondary" role of Scotland in Great Britain. If this presumption is accepted, then the reason lies in the fervid loyalty of Scots to causes, the leaders of which have felt no need to placate these true believers. Liberal and Labour leaders have felt it unnecessary to woo the Scots; Conservatives have felt depressed about the effort. In such a political situation the re-establishment of the office of Secretary of State for Scotland has little meaning unless the office were held by a Tom Johnston, who could use the situation of World War II to force government attention upon uniquely Scottish matters. Once the war was past, however, these matters received only languid notice from either Conservative or Labour parties. The author continues to flog the Argathelians and Henry Dundas, but their suavely corrupt eighteenth-century arrangements forced British governments to consider at least some Scottish interests. Nor is the author particularly convincing when with strange *Schadenfreude* he says that Scotland may have the new role of teaching England how to live as a second-class power. Everything in the book points to a continuing direct ratio between England and Scotland, so that English decline scarcely means Scottish stabilization.

University of Southern California COLIN RHYS LOVELL

DR. JOHNSON'S LONDON. By *Dorothy Marshall*. [New Dimensions in History: Historical Cities.] (New York: John Wiley and Sons. 1968. Pp. xiv, 293. Cloth $6.95, paper $3.45.)

IT would be difficult to think of anyone better qualified to enliven a synthesis of recent scholarly work on eighteenth-century London with wit and imaginative insight than Dorothy Marshall. *Dr. Johnson's London* will disappoint her many admirers, however, for it provides neither new facts nor fresh interpretation. London is less the subject of the book than a convenient peg on which to hang a series of essays on different aspects of eighteenth-century English life and

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history. There are chapters on London's topography and growth, economic activities, City and national politics, amusements, scholarly and artistic activity, poverty and crime, and philanthropy. The chapter on "The Politician's London" is representative: a perfectly adequate description of the workings of the Georgian constitution, but one that would have fitted equally well into an ordinary textbook, for it deals only in passing with the effect on British political and constitutional developments of the seat of government being in Westminster rather than elsewhere.

If the specialist finds little that is original, students will find the book of equally limited value as an introduction to more detailed studies. Although there are footnote references for most of the usually delightful but often misquoted source extracts—the author makes good use of the accounts of foreign travelers—there are not always references to secondary works; nor is there a bibliography. She has been ill-served by her publishers, who appear to have dispensed not only with editing but also with the most cursory proofreading. The one inadequate map shows such places as "Picadilly," "Berkley Square," and the "Foundlings Hospital." The first page of the text begins by attributing a quotation from Boswell—and a very Boswellian one at that—to Dr. Johnson. Scarcely a page follows that is not disfigured by some factual inaccuracy, mistake in spelling, or inconsistency of usage; the whole shows all the signs of carelessness and haste that one finds in undergraduate essays produced under pressure of time, but that would not have been expected from the author of *The English People in the Eighteenth Century*. The chapters on poverty and crime and on philanthropy are more worthy of their author than are the earlier sections, and they suggest what a pleasant and useful book this might have been.

Vassar College DONALD J. OLSEN

THE RELIGIOUS THOUGHT OF SAMUEL JOHNSON. By *Chester F. Chapin*. (Ann Arbor: University of Michigan Press. 1968. Pp. viii, 181. $7.50.)

SEVERAL years ago Donald Greene remarked upon the wildly inaccurate ideas some historians have about Dr. Johnson, naming Bury, Brinton, and Trevor-Roper as special sinners. In his book *The Politics of Dr. Johnson* (1960), Greene attempted to show that Johnson was far from the reactionary Tory of legend. It is a part of the same legend that Johnson was a narrow-minded religious bigot. Greene also noted in 1965 that "little work that is adequate" has been done on Johnson's religion, and he lamented the failure to place what does exist in the context of the Christianity of Johnson's time. Literary scholars are better at poring over every line of Johnson and every scrap of biographical information than they are at understanding the political and intellectual currents of his time.

Chester F. Chapin has sought to supply this need, but he remains much more at home with the personal and literary aspects than with the broader setting. Chapin is at one with Greene, and with the current Johnson revival, in rescuing the great epigrammatist from the stigma of being only an eccentric. Johnson was a serious and profound thinker, spiritually as well as intellectually

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great. He was also surprisingly "modern": W. J. Bate saw him as anticipating Freud; Chapin notes his existentialism, his intense "commitment," matched with a tragic sense of life. The utter hopelessness of man's condition staggered Johnson, but he did not doubt, therefore, that life has meaning. This has, however, been said before, and more memorably, as, for example, in B. H. Bronson's "Johnson Agoniste." Chapin has no great gift of style. It is not clear that he adds much that is new to the Johnson lexicon. There is some discussion of the many religious disputes of Johnson's century, from the controversy about the Bible and Christian evidences to the philosophical issues of free will and *unde malum,* but these chapters are too thin to constitute adequate coverage. One may note especially an almost complete neglect of the deistic debate that was so prominent and far-ranging during the very years of Johnson's youth; William Law's impact on him can hardly be understood except against this background. What is said about such major figures as David Hume and Joseph Butler does not encourage confidence that the author has thoroughly assimilated their religious views. Likewise, the complex political situation of this age of equipoise, at a time when religion was still not fully separated from politics, appears to be incompletely grasped. One wishes that this short book had been much longer and provided more extensive examination of such topics, together with a greater depth of understanding. Perhaps the author has fallen between the stools of a literary essay on Johnson and a history of the religious thought of his age. Those historians who are ill-informed about Johnson can still learn much from this book. The index, consisting only of proper names, is inadequate.

University of Wisconsin-Milwaukee ROLAND N. STROMBERG

THE ENGLISH JACOBINS: REFORMERS IN LATE EIGHTEENTH CENTURY ENGLAND. By *Carl B. Cone.* (New York: Charles Scribner's Sons. 1968. Pp. 248.)

As the author of previous works on those famous antagonists, Edmund Burke and Richard Price, Professor Cone is well equipped to survey the stormy history of the English "Jacobins," whom he might have called British since he deals also with Scotland. The present study, which is without references or bibliography, will be of limited use to those seeking new detail. Its interest lies more in the judgments that the author, from his long experience with the period, brings to bear on this early phase of the British radical movement.

Much attention is given to the agitation for parliamentary reform as expressed by Cartwright, Wilkes, and Wyvill in the twenty years preceding the strictly "Jacobin" period, in which reformers were at the same time sympathizers with the French Revolution. The anti-Jacobin reaction is also considered. The result, in a short book, is to have less space for the Jacobins themselves but to put them in broader perspective. The author finds that in the long radical tradition, from the Levellers to the time of the American Revolution, it was assumed even by extremists that only property owners should have the vote. It was a momentous innovation about 1780 for some to demand a full universal male suffrage, with representation by numbers. These first "democrats" were middle- or upper-class intellectuals. A second innovation, by 1792, was the appearance of a genuine

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working-class political agitation, carried on by the London Corresponding Society and similar groups in the provincial manufacturing centers, which were often ahead of London. There was, consequently, a third new phenomenon in British public life: a profound fear on the part of the upper classes for the working people and "lower orders." The former withdrew from the reform movement, which by 1794 was almost purely an affair of small tradesmen and artisans. These English Jacobins, though suppressed, thus initiated the modern working-class movement in England. This view coincides with that of E. P. Thompson's *Making of the English Working Class.* What no writer seems to point out is that the English Jacobins, in their social composition, showed more resemblance to the Paris sans-culottes than to either the real French Jacobins or the quasi-Jacobins on the rest of the Continent, who were predominantly middle-class persons.

Cone is not inclined to overstate the importance of his subject. He thinks that the reform movement was discredited by the number of Dissenters in its midst and might have died out by 1790 had it not been stirred up by Burke's assault on the French Revolution. Paine's *Rights of Man* both spread the agitation and weakened it by associating it with outright republicanism. The author concludes, like previous students, that the English Jacobins were too few and too lacking in resources to achieve their objectives. While rightly insisting that the movement was wholly indigenous, he seems to me to miss the full impact of the French Revolution upon it, and also the full force of the opposition to a war that was widely believed to be unnecessary and outrageous. The tone is too bland to convey the bitterness of these disaffected Englishmen of the 1790's. The significant question is not their lack of revolutionary potential but the scar left on English society by a wound that went so long without treatment.

Princeton University R. R. Palmer

THE LIFE AND LETTERS OF VICE-ADMIRAL LORD COLLINGWOOD. By *Oliver Warner.* (New York: Oxford University Press. 1968. Pp. xix, 276. $5.95.)

Cuthbert Collingwood, a dedicated naval officer of the highest competence, served afloat from the outbreak of war with France in 1793 until his death in 1810, with only a brief year (1802) for relaxation. As a captain he commanded ships of the line on the "Glorious First of June" and in the Battle of Cape St. Vincent. And yet, despite his sixteen years at sea in time of war, many readers of naval history know little more of him than that he commanded and led the lee division at Trafalgar and that, as his flagship opened fire, Nelson remarked to those around him: "See how that noble fellow Collingwood takes his ship into action."

The main reason for this comparative neglect of interest appears to have been a concentration on efficiency that left him little time for the amenities of life. He was neither callous nor unmindful of the welfare of his officers and men, and his behavior was always correct to those serving above or below him, but friendship usually escaped him. He had known and admired Nelson, who was ten years his junior in age, for more than thirty years. And with Vice-Admiral Purvis, his second-in-command in the Mediterranean, his private correspondence was as

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cordial as could be wished. But it was only to a select band of relatives, most of them in far-off Northumberland, that he unburdened himself. Fortunately he was a very good letter writer, and many of his letters, particularly those taken from the *Private Correspondence* published by the Navy Records Society in 1957, enhance Oliver Warner's excellent biography and result in as clear and sympathetic a picture of his hero as one could want.

Much of the book is devoted to those four and a half years after Trafalgar when, without respite, he held the arduous post of commander in chief in the Mediterranean. He had eighty warships under his orders, and his responsibilities were legion. There was much sea-borne trade to be protected, Sicily must be kept secure, friendship with Turkey must be cultivated, and a watchful eye had always to be kept on Toulon lest the French fleet should be roused to activity. All these matters he handled with assurance and complete success, though in the process he wore himself out by long hours at his desk, hardly if at all mitigated by social contact with his officers—contact that, with many admirals, did something to relieve their isolation. When at last the Admiralty agreed that he should be allowed to come home to recuperate it was too late. Within two days of sailing for England he died, being then in his sixty-second year.

Cattistock, Dorset, England JOHN CRESWELL

THE REVOLUTION OF THE DONS: CAMBRIDGE AND SOCIETY IN VICTORIAN ENGLAND. By *Sheldon Rothblatt.* (New York: Basic Books. 1968. Pp. 319. $7.50.)

VICTORIAN Cambridge, Mr. Rothblatt explains, drew most of its undergraduates from the well-to-do middle classes and expected them to enter the professions or government service; it gave little encouragement to young men of very limited financial resources and made little effort before the 1890's, when seemingly the professions began to be crowded and a Cambridge Appointments Association was formed, to bridge the gap between itself and the Philistine world of business. But during the 1860's, when effective instruction had fallen almost entirely into the hands of private coaches, a number of young dons, mindful of the professional ideal, set about redefining their roles within the institution. The "Revolution of the Dons," which may seem relatively mild to those who have witnessed student riots a hundred years later in other places, broadened and strengthened the instruction offered by the university and stimulated new concern for the training of character in the colleges.

Rothblatt takes many pages to shake free of impersonalities and get moving. His first two chapters (and first two appendixes), which have to do with educational costs and closed scholarships as impediments to social mobility, necessarily make use of incomplete statistics and somewhat enlarge the importance of Sidney Sussex College; his third chapter, though it provides some account of Adam Sedgwick and William Whewell as intuitional philosophers at Cambridge, deals mainly with the educational thought and influence of Mill and Matthew Arnold, who were educated elsewhere.

To come upon Henry Sidgwick is to feel the story grow livelier—though it is a bit puzzling that his sexual constitution should claim more attention,

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apropos of revolution, than his interest in Newnham or, for that matter, Maurice's in the London Working Men's College. Reading the next four chapters, about the reforming endeavors of Sidgwick, Jebb, and Henry Jackson at Trinity, and of Austen Leigh and Henry Bradshaw at King's (not to mention the ineffable Oscar Browning), one enjoys Rothblatt at his best and learns more of Cambridge as it was, and has been more recently. A surprising number of the memoirs in the highly organized "Select List of Sources" are of men and women who lived more of their years after the Queen's death than before and were visibly alive, between wars, in a Cambridge that no longer seemed particularly Victorian.

Barnard College D. A. Robertson, Jr.

THE COLLECTED WORKS OF WALTER BAGEHOT. Edited by *Norman St John-Stevas*. Volumes III and IV, THE HISTORICAL ESSAYS. With an introduction by *Jacques Barzun*. (Cambridge, Mass.: Harvard University Press. 1968. Pp. 574; 523. $20.00 the set.)

In his provocative introduction to these volumes of Bagehot's *Collected Works*, Jacques Barzun gives several reasons why Bagehot is less well known than he might be; the explanations range from the difficulty of pronouncing his name, to the difficulty of assigning him to any one discipline, and to the still greater difficulty of attributing to him any simple and single set of opinions. It is to be hoped that the present edition (a contemplated nine volumes) will overcome these and whatever other impediments there may be to a proper appreciation of a very considerable thinker.

Indeed Bagehot may come to suffer from a surfeit of popularity. He is so eminently quotable that the temptation will be to quote him on all sides and to all effect. He was not a conscious purveyor of maxims—that would have been too precious for a man of his character—but his distinctive humanity, wisdom, and style combine to produce something that, taken out of context, bears a suspicious resemblance to the maxim. Of how many a politician might it not be said that, like Peel, he had "the powers of a first-rate man and the creed of a second-rate man"; or, like Palmerston, that he "was not a common man, but a common man might have been cut out of him"; or, like Sir George Cornewall Lewis, that he was too rational and dispassionate for his own good: "Such extreme calmness of mind is not favourable to a statesman; it is good to be without vices, but it is not good to be without temptation."

But apothegms of this sort, and, more important, the kind of sociological-historical analyses that lay behind them, have been available before in the more familiar of Bagehot's essays. Only a few years ago, Norman St. John-Stevas collected the best of these essays for an Anchor edition. What the total corpus of Bagehot's historical work, as assembled in the present volumes, suggests is that his mode of thought was more deliberate, coherent, one might almost say more systematic than his epigrammatic style would lead us to suppose.

Most illuminating and novel is the second of these volumes. Where the first deals entirely with England, the second is concerned largely with the France of Napoleon III and the United States at the time of the Civil War. There could

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not have been a neater demonstration of political alternatives—parliamentarianism, Caesarism, and democracy, as exemplified in those three countries, representing the characteristic modes of accommodation to the conditions of modernity. Bagehot's letters on the *coup d'état* of 1851 (he was then twenty-five) have been reprinted before and have properly attracted much attention, including his famous tribute to the English for having "the most essential mental quality for a free people," that is, "much stupidity," and his criticism of the French for being too "clever," too volatile by virtue of their intelligence, to sustain free institutions. The *Economist* articles now reprinted for the first time, covering the period from 1863 to 1874, carry over some of these themes (Napoleon's "restless, scheming, brooding, cavernous mind"), but supplement them with a critique of enlightened despotism more reminiscent of Mill than of Burke. The first drawback to this system of government, we are told, is that you must "catch your despot," and the species is rare. The second is that the system has the fatal flaw of preventing the "future elevation" and "progressive improvement" of mankind because it denies the free thought and free institutions necessary for progress.

The articles on the United States, also reprinted for the first time, show Bagehot responding even more sensitively to the moods and experiences of those years, sometimes reflecting prevailing opinion (Lincoln immediately after his election: "the nearly unknown man . . . who must, from his previous life and defective education, be wanting in the liberal acquirements and mental training which are principal elements of an enlarged statesmanship"), sometimes sharply differing from it (as when he criticized the Emancipation Proclamation for being "so transparently *un*-anti-slavery"). As befitted the editor of the *Economist,* he discoursed gravely on the commercial consequences of the war for Britain, but he never permitted such economic considerations to obscure what he regarded as the paramount moral issue: the absolute evil of slavery. And he reflected upon the constitutional arrangements that exacerbated the conflict, reflections that were later to find their way into *The English Constitution.*

When Gladstone, after representing Oxford in Parliament for eighteen years, was defeated in the election of 1865, Bagehot recalled that Oxford had earlier rejected both Canning and Peel, "the three statesmen who were in their generations the best products of her teaching, and who were alike in this—that they each at least tried to combine the true Conservatism and the true Liberalism—the inheritance of the past and the necessities of the present." Many thinkers have subscribed to some such generalization. Few have been as perceptive as Bagehot in specifying, in particular situations, what was owing to the past and what to the present, and how these two obligations could best be mediated.

Brooklyn College GERTRUDE HIMMELFARB

THE CHRISTIAN SOCIALIST REVIVAL, 1877–1914: RELIGION, CLASS, AND SOCIAL CONSCIENCE IN LATE-VICTORIAN ENGLAND. By *Peter d'A. Jones.* (Princeton, N. J.: Princeton University Press. 1968. Pp. xiii, 504. $12.50.)

THE title of Professor Jones's study indicates well enough its scope and focus. More carefully and fully than any of his predecessors, the author has charted the

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origins and development of some dozen Christian Socialist societies within and without the Church of England. Jones gives useful summaries of recent scholarship on the impact of Henry George upon English opinion, the vicissitudes of late Victorian economy, and the state of irreligion among the working classes; his conclusion contains some modest and sound generalizations. But for the most part, the book is best characterized by the author's own phrase: it is a "narrative" telling the histories of these societies, once their respective founders and leaders have been briefly described, and their theological and political positions charted. In his research into previously unexploited archives and periodicals, Jones has left little undone. His volume is in one limited sense definitive as a description of Christian Socialism during this period.

Yet it must be said that there is a pronounced disjunction between such attention as he gives to general trends such as that of the economy and the detailed, microscopic mode of proceeding that characterizes Jones's account of the associations he groups together here. It soon becomes clear that the author is not very interested in the sources and varieties of religious experience. His way of dealing with the central figures in his story is to summarize their formal theology or their organizational manifestoes. He does not attempt to achieve an imaginative understanding of why Christianity mattered to them or why they interpreted its social teaching in the way that they did. As for socialism, Jones is at once better informed and more sympathetic, although he awards praise and blame according to some standard that he assumes to be self-evident and, hence, not requiring justification. Nor does he find it easy to forgive men with public-school or aristocratic backgrounds, although they condemned their fellows and their society on religious grounds. Thus the treatment of Bishop Gore is grudging in the extreme, a point significant not so much because of the individual case but rather because of the author's failure to identify and deal with an important group within the ruling class.

Jones begins and ends his book by declaring Christian Socialism to be an "enigma." Apparently he understands this term to mean "inexplicable," for he attempts little or no explanation of why these exceptional Christians cared about the causes they championed and why they took care to distinguish themselves from their unconcerned fellow believers on the one side and secular reformers on the other. Because he lacks sympathy with his subject and because he does not justify such an extended treatment of it, Jones seems to tell us more than we wish to know. This work is the revision of a thesis, and in all too many places index cards are still strung together in approximately chronological order. In such sections, the author's tedium is scarcely concealed; it is infectious. Yet whenever the author deals with his most recent research interests, the book does come to life.

Jones fails to analyze with sufficient care those abstractions used by his subjects, and by himself in his judgment of them. For the most part he uses Dicey's distinction between "individualism" and "collectivism" despite the telling criticisms made of this dichotomy by historians in the past two decades. Thus, at the end of the volume, Jones ventures an assessment of the effects produced by Christian Socialism: it made "substantial contributions to the development of the Fabian Society and the Independent Labour Party" and to the support of "collectivist" measures.

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Among the points of reference Jones used to clarify the positions taken by the Christian Socialists is the political theory of T. H. Green, who taught many of them. Jones believes that Green regarded the state as sacred and thus was a "collectivist." No textual evidence is cited to support such an interpretation. I have argued elsewhere that Green ought not be so classified. He was ambivalent about the role of the state and argued that there is a presumption against action by it when individuals or associations can perform effectively in its place. Green did concede that the state ought to act when it alone could perform a function so necessary that its omission would affect adversely the common good. Such a position cannot accurately be characterized as advocacy of the "positive state." It may be argued that Green was profoundly ambiguous on this entire range of questions. But under some circumstances ambiguity enhances rather than diminishes the attractiveness of a position. And in censuring the Christian Socialists for the vagueness of their thought and political strategy, does not Jones judge them by standards of our time rather than theirs?

Hunter College MELVIN RICHTER

FRENCH ABSOLUTISM: THE CRUCIAL PHASE, 1620–1629. By *A. D. Lublinskaya.* Translated by *Brian Pearce.* With a foreword by *J. H. Elliott.* (New York: Cambridge University Press. 1968. Pp. xiv, 349. $13.00.)

THE author, formerly professor of medieval history at Leningrad University and now a research fellow at the USSR Academy of Sciences, wrote this summary and critique in 1965 to introduce Russian readers to the works of Western historians on the "general crisis of the seventeenth century." The translation, in turn, will bring to Westerners her commentary and her own excellent study of the problems of the French government in the 1620's. As formidable as is the documentation in this closely written volume, more evidence and a greater variety of evidence appear to be necessary in order to analyze definitively the roles of various classes and individuals and their relationships to institutions of seventeenth-century France.

Displaying a most impressive knowledge of the sources, Professor Lublinskaya offers penetrating comments upon a veritable catalogue of works on the seventeenth century, and if the author's straightforward and masterful critique urges that Mousnier's thesis needs qualification, then an equal or greater modification in the conclusions of B. F. Porshnev is suggested. The articles by H. R. Trevor-Roper, however, probably receive her most damaging criticism.

As Lublinskaya offers her own careful analysis and explanation of the government's policy in France from 1620 to 1629, Cardinal Richelieu is reduced to something like life-size and placed in perspective with other ministers and Louis XIII. With the government's failure to win desired financial reforms from the Assembly of Notables in 1626–1627, Richelieu turned back to the old policy of borrowing to meet state expenses. The extremely able cardinal, Lublinskaya asserts, was forced to resort to earlier practices and expedients. "It is, however, beyond question that he achieved unprecedented success. It seems to me though, that this success was decided by the profound changes in French society itself which took place between 1610 and the 1620s." Whether any one decade of the

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seventeenth century was the most crucial of all may be largely a matter of semantics and is probably considerably less important than the contribution Lublinskaya has already made in assembling her strict and perceptive critique and adding her study of the period 1620–1629, a treatment worthy of consideration by every interested scholar. The critique is an extremely valuable summary of the literature on the crisis of the seventeenth century.

Pennsylvania State University ROBERT W. GREEN

BORDEAUX AU XVIII[e] SIÈCLE. Under the direction of *François-Georges Pariset*. With the collaboration of *P. Bécamps et al.* [Histoire de Bordeaux, Volume V.] (Bordeaux: [Fédération historique du Sud-Ouest.] 1968. Pp. 723.)

THIS beautifully presented book is part of an eight-volume series on the history of Bordeaux from antiquity to the present under the general editorship of Charles Higounet. This volume covers the century from 1715 to 1815 and is divided into four distinct sections that might well stand on their own. They are: the political, intellectual, and religious setting; the social and economic structure; the Revolution and Empire; and the *beaux-arts*. The chapters by François Crouzet on the economic history of the burgeoning Atlantic port are among the best in the book. They present a precise analysis of the vicissitudes of the colonial trade and its auxiliaries (shipbuilding, sugar refining, re-exports), distinguishing the period of accelerated commercial growth before 1755 from the increasing uncertainties of the trade with the "isles" after the disastrous Seven Years' War. Although large profits were still made in the 1770's, Dutch and English interlopers and the accumulating bad debts of the planters of Santo Domingo presaged the absolute decline that was to come. Crouzet emphasizes that because of its dependence on the international market, Bordeaux's economic history has no relation to the agrarian price cycles of C.-E. Labrousse. Bordeaux was a "foreign body" in the national economy of France.

J.-P. Poussou's chapter on the social and demographic configuration of the city points to immigration as the prime reason why the population doubled between 1715 and 1789. Here, as at Paris and Amiens, is a city overwhelmingly populated by artisans, domestics, day laborers, and poor with a relatively small number of *marchands* (ranging from lumber merchants to shopkeepers) and *bourgeois* (landlords and *rentiers* with privileges of "citizenship"), and an even smaller number of *négociants* and nobles (together only 4 per cent of a population of 100,000). The *négociants* were the rich import-exporters—Gradis, Bonnaffé, Bethmann, Nairac—57 per cent of whose dowries exceeded 200,000 livres! About a third of these "capitalists" were Protestants unwilling to abandon either religion or trade for the lure of ennobling office. Curious Frenchmen indeed! The robe nobles of the parlement of Bordeaux—Le Berthon, Ségur, Lavie, Montesquieu—were only slightly less affluent. Their revenues were solidly based on the expanding vineyards of the Medoc, Graves, and Sauternes. A separate chapter by R. Pijassou treats all aspects of winegrowing and the development of the *grands crus*.

L. Desgraves's chapter on intellectual life focuses on the dissemination of

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ideas by means of the clandestine press as well as by officially approved channels—academy, club, *salon*. Membership in the academy was divided almost equally between robe nobles and professional men (especially physicians), and not a single merchant or shipper was interested in the academy or, it appears, in any other intellectual activity of the city. Moreover, "new ideas" were more likely to assume the form of treatises on political economy, abridgments of the Encyclopedia, or pragmatic efforts at experimental science rather than an addiction to speculative philosophy or theoretical science. Religious conditions bear some resemblance to those at Angers or Bayeux. The bishop was likely to rely on a resident vicar-general; the cathedral chapter doggedly defended prerogatives both honorific and real, especially regarding the local curés, while enjoying enviable revenues from the tithe on wine and urban real estate; the secular priests and regular clergy bore the burden of education and charity. Yet despite internal tensions, the Church administered what charity there was and maintained a respectable standard of elementary education, while the Jesuits were even pedagogical pioneers, introducing experimental physics into the curriculum of their schools.

The section on the Revolution and Empire does not attain the exceptional quality of the first two parts. P. Bécamps relies largely on political narrative that too often sacrifices an analysis of issues. One searches in vain for a profile of the Girondin "party," the fate of Church and *émigré* property, the echoes of Jacobin social legislation, or some effects of the Revolution on education, the poor, or popular attitudes and values. In the last section François-Georges Pariset, editor of the book, treats urban renewal and beautification. Energetic intendants such as Boucher and Tourny enlisted the architectural talents of Jacques Gabriel and Victor Louis to produce the neoclassic style that dominates Bordeaux to this day. Prolonged effort and considerable financial strain finally reached fruition in the new Stock Exchange and Customs' House, the Place Royale, the Grand Théâtre, new broad avenues, townhouses, and pavilions gracefully decorated with the output of luxury manufacture. This section is complemented by thirty large reproductions and photographs imparting a rich, visual dimension to Bordeaux in its golden age.

Johns Hopkins University ROBERT FORSTER

LA VIE RURALE EN BASSE-AUVERGNE AU XVIII^e SIÈCLE (1726–1789). Volume I, TEXTE; Volume II, PLANCHES. By *Abel Poitrineau*. [Publications de la Faculté des Lettres et Sciences humaines de l'Université de Clermont-Ferrand, Second Series, Number 23.] (Paris: Presses Universitaires de France. 1965. Pp. 780; 149. 80 fr. the set.)

AFTER the works of Goubert and Le Roy Ladurie, here is another excellent addition to the growing library of studies of provincial France in the old regime. Visibly inspired by the tradition of the *Annales,* Poitrineau has attempted to write what Bloch called total history. No aspect of *Auvergnat* rural society escapes his attention.

This is an extraordinarily dense and rich book. If it makes no startling revelations, it confirms with a wealth of detail much of what had previously been

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suggested by other writers, and it does this within the framework of a province particularly poor, isolated, and cut off from the mainstream of economic development.

Poitrineau begins his study with a survey of demographic change. He concludes that the annual increase of population at the rate of .5 per cent after 1740, a total of 30 per cent in the fifty years before the Revolution, resulted from the attenuation of the terrible famines and epidemics of the earlier period. Physiological misery and disease were still omnipresent (it is fascinating to speculate on their effects on personality), but they killed fewer people. Instead there were a tightening of belts, increasing pressure on the land to produce the necessities of life, and much pauperization of the already hard-pressed peasantry.

Demography alone does not explain these trends. Although there were not many "pure" proletarians deprived of all land, few peasants in Basse Auvergne had sufficient land on which to live without seeking a supplementary income through artisanal enterprise, agricultural day labor, or emigration. Given the weight of seigneurial, royal, and ecclesiastical payments, it was all too easy for them to fall into debt but very difficult to get clear. A short harvest was often the *coup de grâce,* after a long period of living at a bare subsistence level. The peasant's ruin was assured by the obligation to buy grain on the market at the same time as his supplementary income was on the decline. If his land was not actually seized for debt, he often had to abandon it for lack of seed, fertilizer, and other necessary capital. Labrousse's schema of old regime economic crises based in agriculture is here amply borne out.

Under the twin pressures of population rise and a growing money economy, the old solidarities of the village family and community broke down. Land was divided up into tiny parcels, only to fall increasingly into the hands of the urban bourgeoisie (especially in areas near cities) and large peasant proprietors. The price of land, rents, and commodities increased enormously, especially after 1770, but wages could not keep pace. Beggary became the normal result of low salaries, loss of land, illness, and age.

The *Auvergnat* peasant was emotive and quick tempered, a condition Poitrineau ties to a pattern of constant undernourishment and nutritional defects. Like his privileged neighbors, he was litigious and sought to defend his class interests, all the more ardently as his world seemed ever on the verge of falling apart. He was conservative and tended to cling stubbornly to the disappearing economic and institutional poles of the security that he knew. Alienated though he was from the lay and ecclesiastical powers, he would hardly have made a revolution had he been left to himself. He and the province with him were rather slow to follow the Parisian lead in 1789.

In a short review, it is almost impossible to give an adequate sense of the major accomplishment this book represents. Exhaustive research and imaginative tying together of the facts combine to make it indispensable to the specialist. And the general reader or student seeking an example of how to write history, particularly history that deals with the less tangible aspects of human relationships, will find several of the chapters, like the one on the nature of the village community, most rewarding.

Columbia University JEFFRY KAPLOW

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THE NEW JACOBINS: THE FRENCH COMMUNIST PARTY AND THE POPULAR FRONT. By *Daniel R. Brower.* (Ithaca, N. Y.: Cornell University Press. 1968. Pp. xiv, 265. $7.95.)

BEFORE 1934 the special relationship between the Soviet Union and the *Parti communiste français* limited French Communism to an artificial existence. Surviving on foreign subsidies and executing sectarian policies designed for the needs of the Kremlin rather than the French workers, the movement had no roots in the historical consciousness of the nation. Even as the depression deepened, party membership continued to decline, reducing the organization to a hard core of militants embittered by life in the social ghettos and domesticated in service to the Comintern. The popular front changed all that. The new Communist line closed the gap between Soviet interests and French traditions; the defense of the Soviet Union became identified with the defense of the French Republic. While old militants obeyed Moscow as before, a tide of new membership, stirred by Jacobin traditions long dormant in the popular mind, entered the party to defend republican liberties. Ultimately the popular front failed the needs of Soviet diplomacy, but changed the nature of French Communism. Electoral victories gave the party "a real stake in existing French institutions," and the new mass membership, more concerned with liberty than revolution, reflected the image of the nation itself. "The Popular Front," the author concludes, "brought together communism and the Third Republic and created a new Jacobinism."

Such is the theme of this competent work, written in the tradition of history as past politics and based on newspapers and party literature, with gleanings from personal interviews and a few Soviet sources. Its clarity and precision will appeal to undergraduate readers, though specialists will find it a familiar story, except for the originality of its interpretations—not all of which will receive unconditional acceptance. Some readers will be troubled by the concept of Neo-Jacobinism, and this is not merely because the old Jacobinism is a more complicated tradition than this work implies. In places Professor Brower affirms that the popular front "altered the nature of French communism" and "changed the party itself." This finally appears, although he does not say it, to be only a neo-Jacobin delusion, shared by those who had "lost sight of the continued dependence of the party leadership on the Comintern." Students who keep this dependence in view will conclude that the old Bolsheviks counted for more than the "new Jacobins," and that French Communism remained during the popular front what it had been before: the party of Moscow in Paris. *Plus ça change. . . .*

University of New Brunswick GILBERT ALLARDYCE

PARLIAMENT, PARTIES, AND SOCIETY IN FRANCE, 1946–1958. By *Duncan MacRae, Jr.* (New York: St. Martin's Press. 1967. Pp. xiii, 375. $10.00.)

PROFESSOR MacRae is a pioneer in the statistical study of legislative behavior and comparative politics. The general thesis of this volume could be summarized as fol-

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lows: The French polity, during the years 1946–1958, consisted of two political subsystems: that of the National Assembly, marked by "immobilism" and a broad consensus among parliamentary leaders; that of the voters and party militants, unhappy with stalemate and compromise and prone to express their dissatisfaction through "surge" movements in the country (Gaullism, Mendesism, and Poujadism) and through cabinet crises provoked by rank-and-file deputies in the Assembly. In the parliamentary subsystem, cabinet instability resulted much more from ideological factors such as ideological splits between and within parties than from personal ambitions, and immobilism resulted from the arithmetic of the situation, whereby the same group of leaders had to participate in successive cabinets supposed to carry out different policies. In the broader subsystem, the well-known social divisions of the French constituted a permissive but not a sufficient condition for France's political differences; the latter had a resilience of their own. MacRae found no strong correlation between social and religious splits among a party's constituencies and that party's contribution to the fall of cabinets, but he found a considerable stability of the major organized parties and little transfer of votes between these; the compartmentalization of French society, its tendency to foster groups that combine only like-minded persons and to multiply barriers that can be overridden (for instance by surge movements) but not overcome are essential to an understanding of French politics. Only surge movements, drawn from varying social and geographical bases, could occasionally break through, but they were mainly protest movements, not contributions to a system of stable, alternating parties. Relations between the two subsystems were dominated by centralization, which overloaded the parliamentary system and put the pressure of militants' dissatisfaction immediately on this system, and by the Assembly's successful resistance to, and absorption of, surge movements.

In analyzing the deficiencies of the Fourth Republic, MacRae distinguishes three problem areas. One is that in which reforms have been or are being undertaken by the Fifth Republic: the rules of Parliament and of executive-legislative relations, and centralization. The second area concerns the party system; MacRae finds that the French public is ready to accept unifying communications sent by the political leaders, either at the national or at the local level; therefore, the peculiarities of the French parties account for the divisiveness of French politics more than the social divisions of the people. A precondition to reform would be a reduction in the number of competing ideologies and a decline in the strength of the old parties. Both have occurred in recent years, yet it would be rash to speak of any lasting improvement in this realm. There is, finally, the area of "national character." What is necessary here is an end to French resistance to interpersonal relations, a decline of the concern for general principles, the establishment of less exclusive social groupings. Recent events discourage optimism in this area also.

MacRae's work is ambitious. It is, of course, of great interest to those students of politics who are concerned with the development of quantitative methods. MacRae's intelligent analyses of legislative and popular votes help disprove various hypotheses about French politics and lead to several conclusions that will not be questioned any more. But two remarks must be made with respect to method. First, inevitably, the statistical method applies only in so far as there

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are statistics available: MacRae is at his best in dealing with the three legislatures of the Fourth Republic, that is, with the parliamentary groups, their relations, and their own splits; he is least satisfying, and most shadowy, in tracing the relations between the two subsystems, especially between grass-roots politics and the behavior of rank-and-file deputies in Parliament, for these data are less susceptible to mathematical treatment. Nor does he contribute much to our knowledge of the relations between society and politics in the broader subsystem for the same reason. Many of his most exciting statements, in the first and last chapters, are derived from observation and reflection, rather than from the complicated statistical analysis. Second, the latter, while invalidating some hunches or assumptions, tends to confirm the findings of the more serious specialists of French politics. Such confirmation is precious, and the greater precision these methods provide is very valuable. I would conclude that advanced statistical techniques, used with sophistication by a man who knows how to ask probing questions, can be a useful supplement to traditional research, but they cannot be a substitute for it.

Harvard University STANLEY HOFFMANN

NAVARRA DE REINO A PROVINCIA (1828–1841). By *Rodrigo Rodríguez Garraza.* [Colección histórica de la Universidad de Navarra, Number 21.] (Pamplona: the Universidad. 1968. Pp. 516.)

RODRÍGUEZ Garraza makes a limited but significant contribution to the nineteenth-century constitutional history of Spain by showing that Navarre did not lose its medieval political liberties (*fueros*) primarily because of liberal doctrinaire insistence upon centralization and equalitarianism, an interesting conclusion for one associated with a conservative school of history. Instead, his research discloses that the attack upon Navarre's autonomous laws and institutions began in 1800, before the advent of liberalism, and it was continued in succeeding years by officials of the absolutist Ferdinand VII. Liberals merely completed the process during the Carlist War. His point is that any modern government official, whatever his politics, when faced with increasing needs for revenue and soldiers, had to eliminate such obstacles as the anachronistic kingdom of Navarre.

This study concentrates on the political process by which Navarre, a medieval kingdom annexed to Castile in 1512, lost its autonomy and became one of the forty-nine provinces of Spain. This process, begun in earnest in 1829, was cut short by the Carlist War (1833–1839), which provided Navarre with a new opportunity to protect its rights, not so much because Carlist rebels fought for *fueros* after 1835 but because these *fueros* (and those of the Basque provinces) became the logical basis for a negotiated peace between two irreconcilable forces. And because the *Diputación* of Navarre had remained loyal to Isabel, in contrast with the Basque *Juntas Generales,* Navarre had an effective bargaining agent to obtain special rights in the Peace of Vergara (1839) and to secure confirmation in the Law of 1841.

The author has extensively utilized government records in Madrid and Pamplona. Publishing in a series edited by his professor, Federico Suárez Verdaguer of the (*Opus Dei*) University of Navarre, Rodríguez Garraza is vulnerable

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to criticism leveled against Suárez: writing only "external" political history, failing to describe even the politicians (and their clans), ignoring the underlying socioeconomic changes that prompted political change. But this must not be classified as one more defense of Suárez' pro-Carlist viewpoint. Rodríguez Garraza's heroes are the liberal officials of Navarre during the Carlist War; handpicked by Madrid, these "collaborators" maneuvered adroitly. And so in the Law of 1841, when Navarre lost its Cortes, tribunals and laws, and customs services—an inevitable loss—it still retained as a province a remarkable degree of administrative and financial autonomy.

University of Washington JOAN C. ULLMAN

CONTRIBUTIONS À L'HISTOIRE ÉCONOMIQUE ET SOCIALE. Volume IV, 1966–1967. [Centre d'Histoire économique et sociale.] ([Brussels:] Université Libre de Bruxelles, Institut de Sociologie. 1967. Pp. 154. 270 fr. B.)

THIS slender volume in the series published under the auspices of the famous Solvay Institute by the Center of Economic and Social History contains four essays. They are of unequal length and value, but have a common background in the social history of Belgium in the nineteenth century. Documentation in all cases is excellent.

The first essay deals with the over-all socioeconomic conditions in 1894, when the first elections based on general male franchise occurred. The essay contains much interesting information on the situation of the working classes in one of the most industrialized countries at the turn of the century. Roughly one-fifth of all Belgians, numbering slightly over 6,500,000, were employed in industry. The average daily working hours amounted to 10, and the median wage was 2.85 francs per day. The inadequacy of compensation becomes strikingly clear when measured against prices of basic food items. In 1891 the average cost of a loaf of bread was 0.31 francs; a liter of milk cost about 0.20 francs, but meat was at 1.88 francs per kilogram and butter at 2.20 francs.

Small wonder that, according to the authorities in West Flanders, workmen rarely ate eggs and meat and never butter, while subsisting mainly on potatoes and bread. It is even less surprising to learn that, in spite of such restrictions on consumption, many household budgets were unbalanced. Thus of 188 households surveyed in 1891, slightly over 100 operated in the red; 77 managed to have a few centimes left over, and the remaining 10 were in exact balance.

Little social legislation had been passed in the previous half century, and such legislation as was enacted in the 1890's dealt mainly with the enforcement of existing laws on factory inspection, accidents, and child labor. Only slowly, and mainly after 1900, were basic laws safeguarding social benefits, such as retirement and sickness benefits, introduced.

The remainder of the essay deals with the positions of the three major parties vis-à-vis the social question. As might be anticipated, the Socialist party was by far the most active and affirmative in the struggle for social improvements. The Catholic party based its position on the encyclical Rerum novarum. Conservative Catholics were far less enthusiastic toward social reforms, however, than their Leftist counterparts such as the group directed by Abbott Daens. The Liberal

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party, too, had its wing of opponents, composed mainly of the laissez-faire doctrinaires, and its social progressives.

The second essay concerns itself with the rather esoteric subject of the immigration of French religious orders into Belgium following the anticlerical legislation of the Waldeck and Combes ministries in the early 1900's. The fairly substantial influx of the new *émigrés* caused some unfavorable reaction among the Belgians. Objections were not confined to socialist circles and papers speaking of "the black invasion." Attacks centered on economic competition, effects on teaching, and tax exemptions on the part of the newcomers. The issue of group immigration was finally taken up in the *Chambre,* and some of the Belgian bishops undertook to restrict it after the Vatican had ruled that the sensitive issue was one for the Belgian episcopate to decide.

The third essay has as its subject socialist municipal politics in three communes of metropolitan Brussels. Since the mid-1880's representatives of the industrial operatives entered municipal councils in increased numbers. These representatives, according to the author, quickly adapted themselves to their new duties. Their previous work in benefit associations helped them in their effective performance. The concluding essay is somewhat technical in nature, dealing with the public water supply in Brussels between 1830 and 1870. The need to increase and improve this supply rose with the increase in population, and one important aspect was water purification. Brussels experienced cholera periodically, but the city had a lower typhoid rate than many other European capitals. Yet another primary concern was the financing of a new water system, but after mid-century Brussels succeeded in becoming the model for other growing Belgian cities.

American University F. G. Eyck

SUOMEN ALEMPI SOTILASPÄÄLLYSTÖ 1500–LUVUN LOPPUPUOLELLA (N.V. 1570–1600). By *Kyösti Kiuasmaa.* [Historiallisia Tutkimuksia, Number 73.] (Helsinki: Suomen Historiallinen Seura. 1968. Pp. 255.)

This is a monograph about military officers of junior rank in Finland during the three last decades of the sixteenth century. Lengthy conflicts and wars characterized this period in the Baltic area, and the author attempts to unravel some of the changes that they brought about in Finnish society. His stated purpose is to analyze the means and methods the crown or its representatives used to change the nature of the military administration and leadership in time of war. Kiuasmaa concentrates on organizational matters, but he also contributes to knowledge of the social composition and position of the junior ranking officers. He is particularly concerned with the relationship and interaction between this group and civilian officials of roughly corresponding rank and duties. The frequency of transfers among different branches of governmental service, including movement within the armed forces, and the comparisons of remuneration in these spheres of activity are discussed in considerable detail.

The author concludes that organizational innovations necessitated by the long period of warfare resulted in a considerable increase in officer vacancies and contributed to social mobility by offering new opportunities for advancement in service to commoners.

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The military affairs of Sweden and Finland in the sixteenth century have been the object of many studies in both countries. With the exception of the work of two Swedish scholars, Hjalmar Börjeson and Georg Hafström, on Swedish naval officers in the sixteenth century, the lower ranking or junior officer-level military personnel, however, has been little examined. Kiuasmaa's book bridges this gap for Finland. His study amplifies and sharpens the picture of Finnish sixteenth-century society, which several scholars—Pentti Renvall's studies come first to mind—have carefully constructed in past years.

The author has made extensive use of unpublished archival material both in Helsinki and Stockholm. Thorough though it is within the limits that the author has set for himself, the discussion at times is a bit too redundant and verbose; additional editing and compression could have increased its clarity. It includes a German summary that may be consulted by those interested in sixteenth-century military, administrative, and social history.

University of California, Santa Barbara PEKKA KALEVI HAMALAINEN

ESSAYS IN SWEDISH HISTORY. By *Michael Roberts.* (Minneapolis: University of Minnesota Press. 1967. Pp. ix, 358. $8.50.)

SWEDEN AS A GREAT POWER, 1611–1697: GOVERNMENT: SOCIETY: FOREIGN POLICY. Edited by *Michael Roberts.* [Documents of Modern History.] (New York: St. Martin's Press. 1968. Pp. vi, 182. $5.00.)

MICHAEL Roberts has written several works on Swedish history, and the two volumes reviewed here might be joined with two others: *The Early Vasas: A History of Sweden, 1523–1611,* and the two-volume *Gustavus Adolphus: A History of Sweden, 1611–1632.* His interest in Swedish history began before World War II and was reinforced by service in the British embassy in Stockholm. His insight and perceptive analyses of Swedish history owe much to his service and research but more to his understanding of Swedish traditions and customs. These resources cannot be matched among current historians, and only R. Nisbet Bain knew or has done so much for the history of this small country.

In the *Essays* Roberts writes mainly on Gustavus Adolphus and the period immediately following, with two essays on the eighteenth century, one "On Swedish History in General," and a third on the role of the aristocrats in the development of Swedish constitutionalism. These essays have all been published before, but Roberts conveniently brings them together in his effort to acquaint the American and the British public and professional historians with the intricacies of Swedish history.

The *Essays* center on Gustavus Adolphus, his reforms, his political objectives in Germany, military affairs, military strategy, politics, and the British interest in the Baltic during Cromwell's period. In each the writing shines, the well-turned phrase dominates, and the difficult task of explication and explanation is made to appear easy (which it is not). If there are doubts as to the contributions of the work, it would be, from my limited sight, in dealing with Charles XI and Queen Christina; the flaw in the former is in painting far too black a picture of his capabilities and in the latter in giving far too little space to the Queen and her beliefs and motives in the great crisis of 1650 when there was a possibility of a genuine *re-*

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duktion (crown resumption of possession of lands given to Swedish nobles and others for service or other considerations). But the deft handling of complicated themes is well illustrated in the essay on British interest in the Swedish revolution of 1772 where the source materials reveal English concern with those political changes in the Baltic that might enhance French influence or allow Russia to gain predominance over other powers. Roberts takes the view that British policy followed too closely on public opinion in this revolution, and too little attention was paid to the common interest of Russia and England in preservation of the *status quo,* a view that Harris, the British minister at St. Petersburg, dinned into Russian ears when the League of Armed Neutrality was being negotiated. These *Essays* are a significant contribution to Swedish history, with the first helping to explain why the history of this small country is significant.

In a similar vein the documents in *Sweden as a Great Power* emphasize the nature of domestic quarrels and the limitations upon Sweden in achieving its goals between 1611 and 1697 ("Storhetstiden"). The documents on the government allow Roberts to note the constant enmity of crown and nobility and the uneasy alliance between crown and peasants. In this epoch Sweden moved, by governmental reform, into near absolutism under Charles XI, which the documents clearly illustrate, often with ludicrous clarity, when members of the House of Nobility are quoted directly. In the second section on "Society," the central themes are the crown's attempt to protect peasants and the social issues of the Great *Reduktion* of 1680. It could appropriately be labeled a political chapter, for it concentrates on the rights of the peasantry and the alliance of king, peasants, burghers, and a portion of the clergy against their snobbish overlords in the House of Nobles and those clerics in "high seats." The third section deals with problems in Swedish foreign policy: the war with Poland, Gustavus Adolphus' intervention in Germany, the war with Denmark and territorial aggrandizement for Sweden, and the attempts to preserve Swedish territorial gains after 1660. This is a worthy theme, and Roberts handles it with skill and care. The documents for all three sections are carefully selected, available only in Swedish, and they serve very usefully the student of history who wishes to know trends outside the major countries.

Swedes and Americans, British and historians from other countries, will be delighted with these additions to the sparse literature on Sweden. With his work on the Vasas, Roberts has now moved from 1523 and Gustavus Vasa to the death of Gustavus Adolphus on the battlefield at Lützen in 1632. In the future he plans to write on the remainder of the century, omit Charles XII, whose biography by Ragnhild Hatton soon ought to appear, and continue into the eighteenth century. This ambitious vision Roberts alone can achieve, and so we look forward to further works from his competent and facile pen.

California State College, Long Beach RAYMOND E. LINDGREN

THE ALAND ISLANDS QUESTION: ITS SETTLEMENT BY THE LEAGUE OF NATIONS. By *James Barros.* (New Haven, Conn.: Yale University Press. 1968. Pp. xiii, 362. $10.00.)

IT is nearly a quarter of a century since the founding of the United Nations and

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half a century since the launching of the League of Nations, but obviously the organization of international collective security remains elusive and probably remote. Yet no student of diplomatic history would hold that either of these bodies has been without influence in the peaceful accommodation of a considerable number of international conflicts. Most dramatic have been those instances when it has contributed to the halting of actual fighting, but not to be overlooked are those occasions when League and UN provided procedures and an atmosphere conducive to diplomatic, in contrast to military, means of settlement: Upper Silesia, Macedonia, Leticia, and Mosul for the League; Azerbaijan, Lebanon, West Irian, and elsewhere for the UN. The Aland Islands dispute, the very first example of this latter function, is the topic of this book.

Sketching the early history of the Aland Islands controversy from the Treaty of Nystad, the author recounts in detail the Alanders' 1917 bid for Swedish annexation, Stockholm's appeal to the Allied Supreme Council, the Finns' dogged opposition, and the role of the League as mediator in the accommodation reached in 1920. Professor Barros' principal contribution is his analysis of how the newly created international organization furnished an arena within which the conflicting interests of Alanders, Swedes, Finns, and the Great Powers were reconciled. At no time was resort to arms entertained, consequently coercion in the name of collective security was never contemplated, but Barros demonstrates that the good offices of the League were still important in providing an alternative path for negotiations that bilaterally had reached a dead end. A second attribute is the work's comprehensive approach. For this reason the author is not faulted for retracing without essential addition the research on this subject by the Finnish scholar, Juhani Paasivirta, for the years 1918–1919. Of more concern to Scandinavian specialists will be his bibliographical omission of Paasivirta's *The Victors in World War I and Finland* (1965). The book provides, nevertheless, a sound account of an international controversy whose origins are properly placed in the eighteenth century and whose resolution was one of the few fine hours of the League of Nations.

Pennsylvania State University KENT FORSTER

SPIRIT VERSUS STRUCTURE: LUTHER AND THE INSTITUTIONS OF THE CHURCH. By *Jaroslav Pelikan*. [Character in Crisis.] (New York: Harper and Row. 1968. Pp. x, 149. $4.95.)

AWARE of the current interest in the relation between basic commitments and the institutions that embody them and convinced of the relevance of the Reformation for today, Professor Pelikan takes a look at Luther from a new perspective and asks new questions concerning his character and doctrines. Specifically, he examines significant statements of the reformer during the two critical decades following the publication of his ninety-five theses to ascertain why he believed that the structures of the Church were not adequate vehicles for the Spirit of God and the spirit of man.

In search of his answer, Pelikan analyzes Luther's attacks on the inherited ecclesiastical establishment as they were related to specific controversies over the structures in which he himself was personally involved. He begins with the

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pamphlet, *The Babylonian Captivity of the Church* (1520), in which Luther declared his independence of the Church because he believed the medieval prelates had crushed the Spirit in its institutions, especially in the sacraments, and continues with the treatise *Concerning the Ministry* (1523), which contains Luther's rejection of the inherited priestly structure and outlines one based solely on the Bible; *The Judgment of Martin Luther on Monastic Vows* (1521), in which the reformer attacked one of the institutions most characteristic of medieval life and responsible for such important practical services as missions, social welfare, and education; the essay *Concerning Rebaptism* (1528), in which he defended infant baptism as being not just a medieval papal structure but also an instrument of the Holy Spirit; various attacks on the *Corpus of the Canon,* which he had consigned to the flames in 1520 but to certain features of which he referred in attempting to restore discipline, as in his *On Marriage Matters* (1530); and his *Smalcald Articles* (1539), in which he outlined his views concerning the sacramental system, namely, the presentation of the Word of God through preaching the Gospel and administering the sacraments.

Since the chapters of this book originally were lectures delivered in Bratislava and Warsaw, the book has a special interest for us. There are some repetitions which Pelikan tries to iron out by the use of cross references in the text. The study is a substantial contribution to our understanding of Luther, who began his career as a reformer by supposing that the inherited structures were an obstacle to the life of the Spirit and who gradually came to the conclusion that they could and should be made effective channels for the Spirit, but only in an *ecclesia semper reformanda.*

Ohio State University HAROLD J. GRIMM

SCHRIFTEN UND BRIEFE: KRITISCHE GESAMTAUSGABE. By *Thomas Müntzer.* With the collaboration of *Paul Kirn.* Edited by *Günther Franz.* [Quellen und Forschungen zur Reformationsgeschichte, Number 33.] ([Gütersloh:] Gütersloher Verlagshaus Gerd Mohn. 1968. Pp. 591. DM 96.)

THE several writings of the radical Protestant reformer Thomas Münzer have been available in a variety of places, from the modernized (and popular) version of H. Brandt to C. Hinrichs' competent edition of the political tracts. A comprehensive, scholarly edition has been missing, and this need has now, at long last, been supplied, almost forty years after the project was first commissioned. *Habent sua fata libelli!*

The volume includes three basic categories of writings: those actually published during Münzer's lifetime, his correspondence, and incidental manuscript materials. In the first group are Münzer's liturgical writings (especially the *Deutsche Kirchenamt* and the *Deutsch-evangelische Messe,* the first two church orders of the Reformation), which are here reprinted with full musical notations. The new edition of Münzer's correspondence is a reprint of an earlier edition by Böhmer-Kirn entitled *Thomas Müntzers Briefwechsel* (1931).

Of special interest will be Sections III, V, and VI of the volume, for here are materials (mostly fragmentary) that have heretofore been either al-

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together unavailable or accessible only at obscure places, such as a *Diplomatische und curiöse Nachlese,* published in 1732. As far as I can see, however, no "revolutionary" materials have emerged.

Münzer scholarship has been very lively during the past decades; Marxist historians have written prolifically, and the first English-language biography was published in 1967. This suggests that the absence of a critical edition of Münzer's writings has not handicapped research. Thus, the potential contribution of the present edition is difficult to assess. In any case, the convenient availability of the entire Münzer corpus within the covers of a single volume makes for a significant addition to the source editions of the sixteenth century.

There is no general introduction to the volume, and the introductions to the individual pieces confine themselves to summary statements on matters pertaining to the printing of the respective tracts. The early High German glossary included in the appendix is succinct but adequate.

Duke University HANS J. HILLERBRAND

ANFÄNGE TÄUFERISCHER GEMEINDEBILDUNGEN IN FRANKEN. By *Günther Bauer*. [Einzelarbeiten aus der Kirchengeschichte Bayerns, Number 43.] (Nuremberg: Selbstverlag des Vereins für bayerische Kirchengeschichte. 1966. Pp. xv, 190. DM 21.)

IN this study, based chiefly upon sixteenth-century sources, both manuscript and printed, Bauer traces in detail the formation and destruction of Anabaptist congregations in Franconia in the decade following the Peasants' War. Beginning with the missionary activity of Hans Hut in the vicinity of Königsberg, the author describes who did what and when and where it was done. He devotes particular attention to the following centers: Alterlangen-Erlangen, Uttenreuth, Schalkhausen, Crainthal, and Nuremberg. For each center he includes brief sections on Anabaptist congregational life, organization, and relationships with other Anabaptists, both inside and outside of Franconia. He clearly points out that, contrary to the fears of the magistrates, these Anabaptists, while retaining an eschatological orientation, rejected the violent revolutionary doctrines of Münzer. They also must be distinguished from the visionary sect to which some former Franconian Anabaptists were attracted in 1530–1531. Bauer finds that the leadership came primarily from self-taught laymen—peasants and artisans—ignorant of medieval mystic and apocalyptic literature. They sought to live after the precepts of the Sermon on the Mount and to establish congregations on the apostolic pattern, a goal Bauer regards as an anachronism that must fail before the established institutions of church and state. Still, Bauer sees the severe persecution and destruction of Anabaptist congregations by both Lutherans and Catholics as evidence of the persecutors' own weaknesses and inner insecurity and as a cause of the rise of the visionary sect.

Footnotes are numerous and for the most part satisfactory, although in one or two places the reader finds an abbreviated reference not previously given in full. There is also one note that does not indicate clearly which of two works by one author is being cited. A few sources cited in the notes are omitted from the bibliography, which contains some items having little if any direct relevance to

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the specific study. Perhaps Bauer intended a select bibliography on Anabaptism in general, but, if so, the ignoring of all scholarship published outside Germany points to serious limitations.

Except for the debatable judgment that the Anabaptist goal was anachronistic, Bauer's conclusions are well supported by his evidence. This work adds to the growing number of detailed studies of Anabaptism in specific areas and will be useful to specialists in future treatments of Anabaptism in general; it is not likely to appeal to the general reader.

Berea College FRANK J. WRAY

THE FRANKFURT PARLIAMENT, 1848–1849. By *Frank Eyck*. (New York: St. Martin's Press. 1968. Pp. xii, 425. $14.50.)

ASSERTING that there has been, up to now, no full-length study of the Frankfurt Parliament, Frank Eyck concentrates on the history of the Parliament itself, from its beginnings through its final disintegration. He keeps a steady focus on the day-to-day activities of the elected representatives at Frankfurt, as they reacted to immediate pressures and as they developed and modified party positions through interaction with each other and in response to happenings in the German states. The result is a work of substance that succeeds in clarifying the problems involved in German unification and the reasons for the Parliament's failure. The author displays a wholly admirable balance of mind, grasp of fact, and understanding of motive.

One may recognize that the author does what he set out to do and still regret that his method results in taking the Parliament somewhat out of the larger context. The situation outside St. Paul's Church remains dim; the Parliament, viewed apart from the larger movements of society, seems somehow bloodless. But perhaps this is precisely the effect Eyck intended since he more than once speaks of the "air of unreality" at Frankfurt. Some of the disembodied quality may also come from an almost total lack of reference, in the body of the book, to the relevant secondary literature. Doubtless considerations of space are explanation enough for this self-limitation, but the result is an unavoidable diminution of depth and interest. One might also criticize the way in which the author speculates as to the motives and understandings of the delegates. Eyck is verbally scrupulous in distinguishing between fact and surmise, but a succession of phrases such as "it may be," or "X may have thought," creates uncertainty as to the extent to which speculation is rooted in the evidence rather than representing only the guesses of an intelligent authorial observer. Lastly, Eyck's style, while lucid and correct, is too often pedestrian. One does not require baroque splendors, but one can ask for more than a numbing succession of short, declarative sentences.

What does the book offer that is new? As to subject matter, it provides considerable and painstaking analysis, based where possible on statistics, of the elections to Frankfurt, the size and the geographical and professional bases of the parties, and the voting on selected crucial issues. Some important debates are fully reported and analyzed. All of this is highly valuable; I would only question the treatment of professional and class origins as betraying a certain superficiality and perhaps unsureness. While the investigations yield nothing that

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requires radical revision, they do enlarge upon and refine the accepted picture. Somewhat the same can be said of Eyck's general conclusions. Others have pointed to the Left's lack of realism, have emphasized the strength of particularism, and have argued that the state governments had their own case, and a good one, against Frankfurt. Eyck's conclusions, however, have added force because the reader can follow closely the process by which they are shaped from patiently accumulated and meticulously examined detail. These judgments, moreover, have been joined together to form a judiciously qualified, thoughtful, and coherent interpretation.

Cleveland State University LENORE O'BOYLE

AUGUST BEBEL: SEIN LEBEN IN DOKUMENTEN, REDEN UND SCHRIFTEN. Edited by *Helmut Hirsch.* (Cologne: Kiepenheuer & Witsch. 1968. Pp. 436. DM 28.)

ALTHOUGH we are still waiting for a definitive biography of August Bebel, the patriarch of German social democracy, we may appease our appetites on what is by now almost a surfeit of hors d'oeuvres. In the last decade or so, there have appeared three biographical sketches (Blumenberg, Bartel, and Schraepler), two editions of correspondence (with Engels and with Victor Adler), a comprehensive Bebel bibliography (Schraepler), and, with the present volume, two editions of selected speeches and writings (the other edited by Albrecht Langner in 1967).

The editing of this work, by the veteran historian of European socialism, Helmut Hirsch, reveals impressive scholarship, yet the result is of little use to scholars. The book contains long selections from Bebel's memoirs (readily available elsewhere) but only brief excerpts from his more obscure publications. There are about a dozen speeches before the *Reichstag,* party congresses, congresses of the International, and so forth, but most of them have been abridged. The editor has augmented this material with innumerable short Bebel quotations that are often excellent but that follow one another chaotically in no recognizable topical or chronological order. Since there is also no subject index, the reader looking for Bebel's views on a particular question will find himself utterly frustrated.

Such assorted documents as Bebel's police record and his birth and marriage certificates are included, together with about twenty appreciations of Bebel by personalities ranging from Bismarck to Stalin. There is also an attractive selection of photographs. Hirsch has attempted to construct a biographical portrait of his subject through documents, speeches, and writings. The result will appeal more to socialist antiquarians than to serious historians.

University of Pittsburgh RICHARD N. HUNT

ZUR GESCHICHTE DER GESCHICHTSSCHREIBUNG. By *Friedrich Meinecke.* Edited and with an introduction by *Eberhard Kessel.* [Friedrich Meinecke Werke, Volume VII.] (Munich: R. Oldenbourg Verlag. 1968. Pp. xxxviii, 486. DM 44.)

THIS weighty volume completes the edition of Friedrich Meinecke's *Werke* in a gratifying manner. Eberhard Kessel, professor at the University of Mainz and

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one of Meinecke's disciples, had earlier edited the writings of his teacher on the theory and philosophy of history (*Werke,* Volume IV). Now he has gathered a rich harvest of essays, addresses, reviews, and obituary articles published in the course of six decades. Many of them had been written for the *Historische Zeitschrift,* which, under Meinecke's brilliant editorship, attained even greater fame than it had possessed under its founder, Heinrich von Sybel. Kessel's selections bring back to the older generation many memories from a great chapter of German historiography, while younger readers who previously had no easy access to these papers will be impressed by the wealth and the depth of Meinecke's observations. This is not a systematic history of modern historiography, but it supplements George P. Gooch's *History and Historians in the Nineteenth Century* in many respects. Meinecke, however, differed from Gooch by limiting himself to German and Swiss scholars, a fact that Kessel justly regrets in his elegant introductory essay.

The volume is divided into five parts: The first is devoted to Meinecke's fundamental aims as a historian. He summed them up superbly in his brief inaugural address given before the Prussian Academy of Science in 1915. His interests, he said, had turned from personalities to ideas; it tempted him to investigate them in an ever wider and more universal framework, yet in close coordination with the hard realities. In some respects, the second part of the volume carries the greatest weight, because it contains some of Meinecke's deepest reflections on Leopold von Ranke and Jacob Burckhardt, whom he considered the two greatest historical thinkers the German *Kulturnation* possessed in the nineteenth century. This part culminates in the text of Meinecke's now-classic address on Ranke and Burckhardt (1948). While Ranke had remained his *Leit- und Polarstern,* he asked himself whether Burckhardt might not become more important than Ranke for the historian because he had understood the character of his own era more deeply and had shown more awareness of coming trends.

Meinecke had started his career in the shadow of three notable political historians. He had taken a course with Johann Gustav Droysen in *Historik.* He worked under Sybel in the Prussian archives and was his last editorial associate on the *Historische Zeitschrift.* His expressions of admiration for Heinrich von Treitschke, with whom he was also connected briefly in the *HZ,* seem dated now; they are of biographical interest because they show the long road Meinecke traveled in half a century. The fourth part concerns the statesman as a writer of history, and these essays on Bismarck, Bülow, and others are somewhat conventional. But the long last part will richly reward the reader because it is devoted to many key figures of modern historiography, colorful men like Max Lehmann and Hans Delbrück; there are also fine observations on Gustav Schmoller and Max Weber. Perhaps the most affectionate eulogy was reserved for Alfred Dove, his predecessor at the University of Freiburg. Meinecke, who at the height of his career had become a genuine liberal, showed courage after Hitler came to power. He dared to support, in the *HZ,* Hermann Oncken, his closest friend in his later years, against the vicious attacks by Walter Frank, the powerful young National Socialist historian who then ended Meinecke's editorship. It is regrettable that the volume does not

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contain one of Meinecke's other tributes to Oncken, but I hope to fill this gap before long. This is, however, the only flaw in an otherwise admirable publication.

Trenton State College FELIX E. HIRSCH

FRAU LOU: NIETZSCHE'S WAYWARD DISCIPLE. By *Rudolph Binion.* (Princeton, N. J.: Princeton University Press. 1968. Pp. xi, 587. $15.00.)

THIS psychological study of Lou Andreas Salomé is an important event in historical scholarship. She is a fascinating woman in her own right. She was charming and dynamic, close to Nietzsche, Rilke, and Freud, and a friend and confidante of many other German writers and scholars. Professor Binion has transformed her autobiography into a psychological case study of an active mind at work throughout a lifetime. This book combines the most detailed and painstaking scholarship with deep insights into her personality.

Lou was a prolific author of novels, reviews, and essays, which provide the principal subject matter for this study. Binion calls these works "semimemoirs," and his main problem was to unravel the method by which she translated her latent dreams into the manifest tales. His last chapter, "Beyond Frau Lou," gives a clear account of the difficulties that haunt such a procedure. All memoirs are distortions, but this book demonstrates how such memoirs can be used to unravel layers of meaning which, checked against each other and against the records of events, can lead us nearer to an understanding of the actual historical situation and the functioning of the particular personality.

Though the encounter with Nietzsche was central in Lou's life, Binion proves that her own account of this friendship is as false as that which Nietzsche's sister spread abroad in the world. Lou's Electra complex, her need for surrogates, and her fantasies, which made her dominant in all relationships, are persuasively documented.

Binion is correctly cautious when he discusses how she influenced her friends and lovers, as she herself believed. But even if Rilke was dependent upon her for most of his life, did this vitally affect his poetry? Surely Martin Buber derived his "descent into the depth" from other influences than hers. Nor can one be quite convinced that Nietzsche's lines on women in *Zarathustra* are linked to his experience with her. This is possible, but such considerations create one serious methodological problem. Nietzsche's attitude could be derived instead from a widespread feeling at a time when the role of women and the family were under intense discussion. Similarly, Lou's and Blüher's concepts of sexual energy were part of a vast literature that arrived at identical conclusions. The book raises the problem of reducing general phenomena to individual experience. Can we follow Lou in reducing a philosophy to a personal dossier on its author?

Binion criticizes Lou for seeing Nietzsche in this way, but his approach raises the same danger for he has to play by Lou's rules of the game, which he has established. Lou, we are told, went the *Zeitgeist* way with no guidance from the *Zeitgeist.* Yet this guidance was implicit in the problems she faced if perhaps not in the way she solved them, though her thought seems to lack much

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originality. One wonders if her mind always moved on such allusive heights. Little is said about how she lived and who financed her constant and neurotic travels. Such realities only enter with the inflation when she supported herself by practicing psychoanalysis and accepting money from Freud. Binion is aware of these problems, but he believes that an analysis such as he has undertaken comes nearer to historical reality than the more common schematizations and abstractions usually found in intellectual history.

He seems correct in this particular case, and his book must stand together with Erik H. Erikson's *Young Man Luther* as a pioneer study. Historians may be critical of the methods necessary to penetrate into the human mind, particularly the interpretation of allusions and the pinpointing of latent associations. This book uses a Freudian scheme, though its conclusions go beyond some Freudian concepts. Lou is the patient, as seen through her writings, and she is a very loquacious one. Psychology has added a new dimension to historiography, although too many modern biographies are written without taking this into account. Binion, through his study of Lou, has helped to propel us further into the twentieth century.

University of Wisconsin GEORGE L. MOSSE

KURT TUCHOLSKY AND THE ORDEAL OF GERMANY, 1914–1935. By *Harold L. Poor.* (New York: Charles Scribner's Sons. 1968. Pp. xii, 285. $7.95.)

To Americans of the late 1960's the problems of Weimar Germany have become painfully "relevant," as our students would say. The career of Kurt Tucholsky is a case in point. Tucholsky, the foremost social critic and satirical gadfly of the Weimar Republic, suffered throughout his short life (1890–1935) from a dilemma common to the modern intellectual: simultaneous alienation from but intense involvement with the social order in which he lives. The result is often a loss of perspective.

This biography, the first substantial treatment of its subject in English, does an excellent job of analyzing Tucholsky's *Hassliebe* for Germany. He grew up hating the caste-ridden authoritarianism and social injustice of the Wilhelmine *Reich,* saw in the Great War the total bankruptcy of the old Germany, and hoped that the Revolution of 1918 would bring a new regime based on political and social democracy. Instead, as Tucholsky and many other Left-wing intellectuals saw it, the Revolution brought a republic that was only a "continuation of the Empire by other means." The old ruling class retained their positions in the real centers of power, so that the Weimar Constitution was as much the "fig leaf of autocracy" as the Bismarckian constitution had been.

Accordingly, Tucholsky, in witty cabaret chansons and in articles for periodicals like *Die Weltbühne,* tirelessly attacked not only the reactionaries but especially the republicans, who had betrayed the Revolution. The only Weimar party for which Tucholsky could feel any sympathy was the KPD, but he could not accept its dogmatism and servility toward Moscow. As a result Tucholsky and many intellectuals like him literally constituted a "homeless left" languishing in sectarian impotence.

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Poor struggles manfully to exonerate his subject from the standard charge that this "homeless left," by dissecting the Weimar regime so effectively, paved the way for still worse. Yet he has to admit that even the Nazis could not have done a better job of character assassination on Paul Löbe, a perfectly respectable socialist *Reichstag* President. Most readers will conclude in the end that the Tucholsky Left was permanently warped by its pre-1914 experiences. By refusing to admit that the republic was not the Second *Reich,* Tucholsky unwittingly helped to prepare for the Third.

A *trahison des clercs* was an important factor in the dissolution of the Weimar Republic. Poor has given us a valuable case study of this phenomenon on the Left, just as Klemens von Klemperer has treated the new conservatives more generally on the Right. The book is well illustrated and equipped with a full scholarly apparatus, even including a list of recordings of Tucholsky's songs and stories. Tucholsky's style is as untranslatable as H. L. Mencken's; fortunately, Poor has quoted extensively from the original German in his notes.

Oberlin College ROBERT E. NEIL

DAS REICHSBANNER SCHWARZ ROT GOLD: EIN BEITRAG ZUR GESCHICHTE UND STRUKTUR DER POLITISCHEN KAMPFVERBÄNDE ZUR ZEIT DER WEIMARER REPUBLIK. By *Karl Rohe.* [Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, Number 34.] (Düsseldorf: Droste Verlag. 1966. Pp. 494.)

KARL Dietrich Bracher has observed that the existence of the *Reichsbanner* refuted the notion that Germany after 1918 was a "republic without republicans." Bracher's judgment is basically correct, but needs considerable explication and modification. This long-overdue task has now been accomplished in a well-written and well-documented monograph. Rohe's study is at once an important contribution to our understanding of paramilitary organizations and of the political Left in the Weimar Republic. And while the author disclaims having written a definitive study of the *Reichsbanner,* it is about as definitive as we are likely to get for some time.

In view of the arrangement of the book, it should be said that Rohe's study is not intended for the nonspecialist. The book is divided into three parts. The first and third parts are primarily narrative treatments of, respectively, the founding and early years of the *Reichsbanner,* and the period from the disastrous elections of September 1930 to 1933. The middle section, over half the book, is an extended topical treatment of the structure and ideology of the organization, the relationship of the *Reichsbanner* to the *Reichswehr,* to the parties, and to other *Bünde.* Among the more important points raised by the author is the problem of a genuinely republican state ideology, and the place of military organization in such an ideology. The *Reichsbanner,* like other *Bünde,* was composed heavily of the "front generation" of 1914–1918, and the effect this had on subsequent discussions in the organization was by no means slight. What distinguished the *Reichsbanner* from other paramilitary organizations, of course, was its dedication to, and complete identification with, the republic. Therein lay the source of many of its difficulties. Rohe correctly points out that the relatively late date

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of the *Reichsbanner*'s appearance in 1924, *after* the great crisis of 1923, suggests that it was intended less as a "protection formation" than as a morale building republican mass movement. While the organization never really had more than about a million active members (not the three million usually claimed), this still made it the largest of all the *Kampfbünde,* a fact which was not lost on the Rightist groups. In defending the republic, however, the *Reichsbanner* was forced to assume much the same appearance and organization as its opponents on the Right in order to rally mass support. It was thus exposed to criticism from the Left (for its "militarism") and from the Right (for its lack of "true national sentiment"). The resulting dilemma was never effectively resolved. A second major problem of the *Reichsbanner* was its attempt to become and to remain a supraparty organization, a "black-red-gold movement" of the three Weimar coalition parties. This was certainly the goal of Otto Hörsing, the temperamental and controversial chairman of the *Reichsbanner,* until 1932, when he was forced out of the organization and the SPD. But what was difficult at the outset became nearly impossible by 1930 by which time, as the author notes, "the Center would not mobilize battalions for the *Reichsbanner;* the *Staatspartei* could not." To the degree that the *Reichsbanner* became a more purely Socialist organization, antagonisms between Right and Left in the *Bund* and a marked tendency toward "bureaucratization" strikingly paralleled similar internal developments in the SPD.

Rohe clearly shows that, like so much else in the stepchild republic, the *Reichsbanner*'s fatal weakness was its dangerous tendency to be overoptimistic and to react to political situations rather than seizing the initiative. As late as the spring of 1933, some *Reichsbanner* leaders were still waiting for the day of reckoning. By then, it had long since passed.

Loyola University, Chicago THOMAS A. KNAPP

ARCHITECTURE AND POLITICS IN GERMANY, 1918–1945. By *Barbara Miller Lane.* (Cambridge, Mass.: Harvard University Press. 1968. Pp. 278. $15.00.)

THIS monograph is the product of careful research in sources that illuminate traditional problems in two academic areas commonly believed to be disparate. The book begins with an explanation of the "revolution in style" just after the First World War. I should note quickly that the author, unlike almost all historians of the architecture of this period, correctly observes that this change in taste was less profound than an earlier transformation "around 1900 that gave rise to the modern movement as a whole." She shows that, though in other countries there were brilliant architects, fertile with projects, those in Germany were the most productive and became the most renowned because they had the lavish patronage of several liberal and radical local governments in Weimar Germany. Necessarily, much of the narrative discusses the waxing and waning influences of the *Bauhaus,* that radical German design school founded by Walter Gropius who led one of the last campaigns in the West to reunify the fine and the so-called practical arts. Mrs. Lane refreshingly departs from the repe-

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titious hero worship of most other *Bauhaus* scholars by showing that this famous institution had distinguished predecessors and competitors in Germany.

Radical German architecture in the 1920's became politicized. After the Great War, which was generally attributed to an evil social structure, some designers devised exalted theories of architecture so grandly embracing that new architecture was rhetorically introduced with accompanying reforming sociologies and Leftist political aims. Since many of those who favored *"sachlich"* (some of the best parts of the book are those discussing what this word meant in the 1920's) architecture were all too visible favorites of the Weimar socialists, it was inevitable that conservatives and nationalists would direct polemics against them. As warring ideologies rushed to their combative climax in the early 1930's, the debate over theories of design became more heated and ominous. The political significance ascribed to the rationalizers of design also made it certain that the Nazis, once they were in power, would close the *Bauhaus,* which had become one of the hottest political-cultural issues of the age.

Less than a third of the text is devoted to National Socialist architecture. It turns out that there was no specifically Nazi school of architecture, despite the devotion of Hitler and his lieutenants to erecting monuments. Various patrons favored various styles. Those *"sachlich"* architects who somehow avoided becoming political issues were patronized to erect for the Nazis some mass housing and some official buildings that are indistinguishable from the ones they might have designed a decade earlier. The architectural *Gleichschaltung,* then, was vigorously claimed, but was impossible to accomplish. Here the author disagrees with and is much more subtle than previous writers on the subject such as Paul Ortwin Rave and Hellmut Lehmann-Haupt.

One must congratulate the author for having come upon such a rich subject and for having pursued it satisfactorily. We learn more about the vigor of Weimar culture and also that the cultural totalitarianism of the Third *Reich* was slovenly. More than one hundred unusual and excellent illustrations fortify the author's theses and account for the book's high price. Its organization is logical, and it is well indexed. Fifty-two pages of bibliography and notes are surely excessive; much of the material in the separately printed notes would have made the narrative more lively. The writer proved her contentions, but her repetitious stating of these contentions became somewhat nettlesome.

I may stray from my task when I say that I feel that the book should have been more stimulating to read. Lane was dealing with exciting architecture and tumultuous politics. Too many minor characters receive serious attention. Major figures should have been given a physical appearance and made to reveal emotions they surely felt in these exciting years. That the flat roof became a major ideological issue surely deserves ironical as well as factual discussion. There are exploitable ironies as well in the contrast between the successful campaign of Germany's architectural nationalists and their limited production. I yearned for the illustrative vignette, the significant anecdote. Lane has written novel and important professional history, but rather dry literature; the materials for writing both were there.

University of South Carolina RICHARD D. MANDELL

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DIE POLITIK DER KABINETTE WIRTH 1921/22. By *Ernst Laubach.* [Historische Studien, Number 402.] (Lübeck: Matthiesen Verlag. 1968. Pp. 344. DM 44.)

ERNST Laubach's study of the two Wirth administrations is the most comprehensive study of any Weimar administration yet undertaken, and it is based on a prodigious amount of research in the archives of both the German Democratic Republic and the Federal Republic. His use of governmental records should influence all those who work in the Weimar period. His monograph should prove to be a prototype for future administrative histories. Weimar specialists will find its documentation particularly useful; indeed, the footnotes often rival the text in interest. The author has judiciously examined the controversial problems of the period and shows his mastery of the sources, but he is extremely cautious in taking positions on these controversies. His research in the archives has produced what appear to be only a few corrections of the major secondary works. He stoutly defends the policies of Wirth and Rathenau, but on events of greatest significance, such as the Treaty of Rapallo, he offers little that is new. His greatest contribution is his painstaking account of the negotiations over reparations, taxes, and inflation, where he charts largely unexplored territory and the archival sources contribute new information and insights. We learn much of the divisions within the governments, of the relationship between Ebert and his Chancellor, and of the divergent paths taken toward "fulfillment" by cabinet ministers and governmental departments.

The Wirth cabinets have recently received considerable attention as scholars have devoted more time to the early years of the republic. Rudolf Morsey's *Die Deutsche Zentrumspartei 1917–1923* devoted 120 pages to the period of the Wirth administrations, and a recent long article by Lothar Albertin in the *Historische Zeitschrift* (CCV [Dec. 1967], 566–627) analyzed a phase of party history that Laubach only touches. Many crucial events took place at this time: the London ultimatum; the policy of fulfillment; the decision on Upper Silesia; the Conferences of Cannes and Genoa; the Treaty of Rapallo; the assassinations of Erzberger and Rathenau; and the Law for the Protection of the Republic. Many of the crucial problems that bedeviled the republic's leaders were tackled by the Wirth governments, and Laubach tries to deal with all of them. Perhaps the chief criticism one could offer is that he has tried to do too much within the confines of a monograph. Although it is not legitimate to ask an author to write another book to meet a reviewer's objections, this work would have been more effective had it been organized topically, had it included significant chapter summaries, and had the author written a longer concluding chapter recapitulating his contributions to and judgments on this period in German history.

Colorado State University BRUCE B. FRYE

DER DEUTSCHE STANDORT. By *Klaus Mehnert.* (Stuttgart: Deutsche Verlags-Anstalt. 1967. Pp. 415. DM 19.80.)

PROFESSOR Mehnert knows his fellow Germans, their history, and their dilemmas well. Years of study in Russia, China, and America have given him added perspective for thinking about Germany in a larger context. In this book he tries to es-

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tablish for his compatriots a more positive orientation than they have generally developed since 1945.

What future is there for contemporary Germany, a strange combination of economic giant and political dwarf? Is there a *via media* between some new ideological inflammation and affluent indifference? His discussion of Germany's past since 1848 describes an accelerating spiral of detrimental political characteristics and decisions; clearly, the German experience with nationalism was not successful. Then Mehnert evaluates Germany's relations with Western and Eastern Europe. Germany cannot relate alone to an Atlantic community, lest that degenerate to nothing more than an uneasy German-American alliance. The history of its relations with Eastern Europe since 1848 is a succession of negative and ineffective associations. Germany's successful economic ventures with former colonial areas have not, since 1945, produced commensurate political influence and fulfillment, and a solution to this problem is needed.

Mehnert's recommendations and prognosis are certainly well intended, but even partial realization of them would be difficult. Germans must undertake a major redirection of their strengths and talents. Learning from their unsuccessful history, rather than being burdened by it, they should turn from the nation-state and incorporate themselves into a federally united Europe that extends westward from Russia's borders. Sociological and educational restructuring must occur in order to mitigate prejudices and prepare the Germans to play a more effective role in world affairs. They must win recognition for qualities of integrity, humanity, and responsibility. Germans should contribute to the greater good of Europe, as Europe should foster the greater good of the non-Western world. A united Europe, composed of thirty-three states, would permit monarchies and peoples' republics alike to cooperate within a parliamentary, democratic framework. A broad common market, booming production, and mobility for all would absorb the problem of German reunification, since both East and West Germany could participate as separate entities and yet find association in the larger European federation.

When Mehnert wrote this book in 1967, he thought that Russia would only grudgingly accept such a European reorientation for its Eastern European satellites, but would eventually recognize the positive advantages of having the German problem absorbed in a viable, united Europe. In light of events in Czechoslovakia since August 1968, one sees how the best of Kremlinologists overlooked potential Soviet reaction to evidences of reawakening European ideas and to German efforts to find a larger European dimension for their future policies. Apparently Mehnert's book is now itself a piece of history.

University of California, Irvine HENRY CORD MEYER

INDUSTRIA, COMMERCIO E BANCA IN LOMBARDIA NEL XVIII SECOLO. By *Bruno Caizzi.* [Studi e ricerche di storia economica italiana nell'età del Risorgimento.] (Milan: Banca Commerciale Italiana. 1968. Pp. 286.)

THIS volume is the latest in a series published by the *Banca Commerciale* of Milan and covering various aspects of Italian economic history during the two centuries preceding the unification of that country. All but one of the nine studies pub-

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lished thus far deal with the economic development of one or another region: two deal with the Neapolitan economy, two with the Venetian Republic (one of these is by the author of the present volume), and one each on Tuscany, Parma, Liguria, and now Lombardy. The single work that is not regional is the first volume of a general Italian economic history from 1861 to 1914.

Caizzi's work is a very detailed and well-documented account of eighteenth-century economic conditions and developments in what has since become one of the foremost industrial areas of Italy, and indeed of Europe. Most attention is focused on the century following the Peace of Utrecht (1713), which for Lombardy meant a transfer from Spanish to Austrian dominion. The discussion consists of four long chapters: the first two deal with industrial developments, the third with public finance and banking, the last with internal and foreign trade.

The end of the long period of Spanish rule left the region very weak both politically and economically. In striking contrast with its former era of prosperity and expansion under the Visconti, its political unity was now almost lost as were a number of its outlying territories and centers of economic activity. Lombardy's new masters in Vienna began by instituting measures designed to help revive the economy of the region. Though well intentioned, these proved to be ineffective or worse since they were based on the supine assumption that what the area needed above all else was to be "protected" against both the outflow of its industrial output and the inflow of competing commodities. Added to this classic mercantilism, moreover, was the perverse and persistent conviction on the part of the Austrian authorities that the economic potential of the region lay chiefly in agricultural production—an error that subsequent history has resoundingly underscored.

The forty or so years following 1713 proved to be particularly trying for the Lombards, who had to labor under an incredible network of official regulations and internal squabbling that made any economic recovery very unlikely. After the middle of the century, however, a number of economic reforms and relaxations of official restrictions began to appear, which made for some gradual economic growth.

Throughout the period, the region continued to suffer from a serious shortage of business capital, originating in part from the inefficient management of public sources and also in part from the unavailability of private risk capital. The latter was worsened visibly by Viennese attempts to make loan funds equally available to all, with the result that those industries (the silk industry is a notable example) in a position to attract loan funds because of their economic potential were prevented from competing freely for such funds.

As the century wore on, moreover, Lombard merchants succeeded only very slowly in disassembling the many suffocating trappings of restrictive policy: high and unequal tariffs on both imports and exports, the exacting of tribute by local authorities on internal commerce, prohibitions on the consumption of luxury goods, and so on.

The author examines not only the major types of manufacturing, financial institutions, and trade, but traces the development of many single firms in each category. By so doing he achieves a sense of particularity and realism often lacking in more general and aggregative regional economic histories.

Fordham University LOUIS M. SPADARO

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PREZZI, SALARI E SERVIZI A NAPOLI NEL SECOLO XVIII (1734–1806). By *Ruggiero Romano.* [Studi e ricerche di storia economica italiana nell'età del Risorgimento.] (Milan: Banca Commerciale Italiana. 1965. Pp. 162.)

Ruggiero Romano has made many important contributions to the field of price history and to the history of Mediterranean commerce in the sixteenth and seventeenth centuries. In the present work he makes a valuable contribution to the history of prices in Italy in the eighteenth century. The study is primarily a presentation of raw data on prices and wages in Naples in the period between the end of Austrian domination in the kingdom of Naples and the beginning of Napoleonic hegemony (1734–1806). Romano presents price series for over fifty commodities. These are for the most part prices of agricultural products, but there are also prices of industrial, mineral, and colonial products. There are wage series for thirty-one grades of labor, agricultural and nonagricultural. The price and wage data have been drawn from the registers of the *Casa della Annunziata* and the Convent of Santa Domenico in Naples. Agricultural prices rose in Naples in the eighteenth century, while some colonial prices fell and industrial prices remained quite stable. Romano finds that the curve of prices rose faster than the curve of wages so that Naples experienced inflationary pressure similar to that in England, France, and Spain in this period.

Romano's price series is more reliable than his wage series. The wage index shows extreme rigidity through the eighteenth century suggesting that nonmonetary factors (payments in kind, fixed contractual rates, and so forth) may have influenced the behavior of money wages. His concluding remarks on the effects of price inflation in Naples are based on very scant data and do not do justice to the price data he has presented in his tables. He does not consider whether real factors—savings and investment, technology, population—rather than monetary factors may have determined the movement of prices and wages in Naples in the eighteenth century.

Northern Illinois University Ira A. Glazier

GIOVANNI RISTORI: DA ILLUMINISTA A FUNZIONARIO, 1755–1830. By *Carlo Capra.* [Pubblicazioni della Facoltà di Lettere e Filosofia dell'Università di Milano, Number 43. Sezione a cura dell'Istituto di Storia Medievale e Moderna, Number 3.] (Florence: La Nuova Italia Editrice. 1968. Pp. xviii, 186. L. 1,600.)

The process of deducing the whole animal from a bone or two is familiar to the paleontologist. In modern history the technique is rarer. Giovanni Ristori was at first an enthusiastic publicist for the late *settecento* intellectual ferment in northern Italy. Then, preferring his convenience to his opinions, he became a bureaucrat, serving Napoleon and Metternich as educationist or jurist. No sketch of him has hitherto existed. Capra had to find a sentence one place, half a dozen letters another, and, from internal criticism of the subject's articles, reconstruct the man.

Ristori came from a satisfactory family, attended the university at Pisa, claimed to have know Pietro Verri and to have corresponded with Benjamin Franklin. He contributed to a succession of literary papers, graduated to political sheets, flirted with Freemasonry, read law, and started an introduction for a

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study of Justinian's Code. His ideas are well epitomized in an entry for the celebrated 1796 Lombard essay contest on "What Type among Free Governments Best Suits the Happiness of Italy?" Here or elsewhere, with uncanny instinct for the essential, he linked poverty and crime and called for demolition of internal trade barriers, taxation of luxury goods, lower prices for food, an end to noble titles, creation of a national literary taste, confiscation of mortmain lands, freedom of worship, abandonment of torture, reform of courts, codification of laws; he generously imitated contemporary domestic and foreign savants. All but twenty-three pages of the book deal with Ristori's career to 1799. Thereafter, like England's semilegendary Vicar of Bray or Italy's real life poet Vincenzo Monti, Ristori tempered his earlier beliefs to keep in the public pay list zenana. No doubt his device was *È meglio piegarsi che scavezzarsi*—better bend than break.

Franco Valsecchi, once when talking about investigators of the *Risorgimento*'s roots, unwittingly justified Capra's moils and toils. "The work of the eighteenth-century Italian reformers, a work which was an expression of our needs, seemed to these thinkers to have exercised an innovating effect that was more intimate and profound, and more consonant with the nature of our country, than the influence that came from foreign invasion." Ristori is important enough to have a small book—this one—written about him. It tells all anybody will want to know. Capra's technique of historical reconstruction, something from nothing, or almost, is ingenious. It will provide a dutiful paragraph in a future appendix to the *Enciclopedia italiana* or in the *Dizionario biografico degli Italiani* when the letter *R* is reached. Thus knowledge is advanced a trifle.

University of Miami DUANE KOENIG

I CATASTI IMOLESI DEI SECOLI XIX E XX. By *Claudio Rotelli.* [Istituto di Storia economica e sociale dell'Università di Bologna, Fonti, ricerche, testi, Number 5. Ricerche sulla distribuzione della proprietà, Number 1.] (Milan: Dott. A. Giuffrè Editore. 1967. Pp. viii, 50. L. 800.)

IN the general flowering of Italian historical studies during the last fifteen years, economic history has not quite kept pace. Within that field, however, one area shows great promise. As long ago as 1936, Professor Luigi Dal Pane began the serious examination of the various *catasti*—"cadastres" or land registers—undertaken by the Italian communes and states for purposes of taxation and administrative improvement. Data analysis of this kind requires collective planning, a fact recognized by the *Consiglio nazionale delle ricerche,* which has provided support for a group of scholars to study the cadastres of the Emilia-Romagna region. Since this was done, many important studies have been published, among which are Renato Zangheri's work in the area of Bologna, Giorgio Porisini's in Ravenna, and Claudio Rotelli's in Imola.

This essay, a companion to Rotelli's earlier study of land distribution and usage in Imola in the seventeenth and eighteenth centuries (1966), analyzes the cadastres of 1812, 1835, and 1925. Like the earlier work, it includes data on land usage and productivity. The main thrust of the study is directed toward land distribution by class, or social group, and by size of holdings. Additional data on animal husbandry, crop rotation, yield, and productivity are provided by Rotelli's

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careful study of the archives of one distinguished and long-standing estate, that of the Counts Pasolini Dall'Orda.

His general conclusions will interest even those who know nothing of this small community in the Romagna. According to the cadastre of 1812, confiscations in the revolutionary and Napoleonic eras had reduced the ecclesiastical domains to about 8 per cent. (In the cadastre of 1778 church lands amounted to one-third of all agricultural holdings in the commune.) In the same period, the nobility, too, lost its predominance in the countryside of Imola; its share fell from about one-half to one-third of the total. This decline came about not through confiscation but through the abolition of feudal limitations on the free sale of land.

Thus more than 40 per cent of the land in the commune changed ownership. Which elements of the "Third Estate" were the main beneficiaries? Rotelli asserts that the peasantry (or rather the *coltivatori diretti*) gained very little. Two other groups began to compete with the nobles as landowners: the more successful practitioners of the liberal professions, especially doctors and lawyers, and merchants, craftsmen, and small manufacturers. The first group, befitting its social and ideological pretensions, divided its activities between professional work, landed interests, and political affairs. It partly replaced the old nobility in the local political structure. The second group displayed little political ambition but considerable ability in accumulating landholdings.

To be sure, the nobility remained an important force in the countryside. (The cadastre of 1925 shows that they conserved much of their strength even in the twentieth century.) But the new bourgeois landowners continued to accept the "life style" and even the political leadership of the more enlightened sector of the nobility. This is the origin of that "bourgeois-aristocratic" class that dominated the political struggles of the *Risorgimento*. Such considerations are beyond the scope of Rotelli's modest and very solid study. This work will, nevertheless, help us achieve a more profound understanding of modern Italian history in general.

John Jay College of Criminal Justice JOHN M. CAMMETT

L'AGRICOLTURA VENETA DALLA CADUTA DELLA REPUBBLICA ALL'UNITÀ. By *Marino Berengo*. [Studi e ricerche di storia economica italiana nell'età del Risorgimento.] (Milan: Banca Commerciale Italiana. Pp. 380.)

PROFESSOR Berengo has published an impressive study of Venetian agriculture from the period of the Austrian Restoration in northern Italy through unification. He has treated his subject with great authority, and he covers a wide range of institutional and economic problems affecting Venetian agriculture in these decades. His study begins with a chapter on the Austrian land census (*catasto*) in the Veneto, the outstanding reform of a half century of Austrian domination of the region, followed by chapters on population, transport and communications, property distribution, agricultural contracts, technology, and a final section on the history of crops. It is a highly original work, based on very extensive research in the archives of Venice and Vienna.

Venetian agriculture, Berengo shows, was among the most backward in Italy

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during the first half of the nineteenth century. The low level of commercial capitalistic development, he finds, was a legacy of centuries of social and political domination of the countryside by the Venetian aristocracy. In the nineteenth century, he points out, the Venetian aristocracy had evolved into a class of absentee landlords uninterested in technical and social change in the countryside.

Berengo does not view agricultural problems against the background of aggregate economic developments, and this limits the effectiveness of his study somewhat. He has avoided all quantitative data on agricultural output in this period on grounds that such data are unreliable. This is an unfortunate omission as it would have been helpful had he placed this material in a separate appendix and allowed the reader to judge for himself. There is indeed great need for such data to explain growing regional inequalities in income in the Italian economy during the nineteenth century.

Northern Illinois University IRA A. GLAZIER

L'ABATE GIOVANNI A PRATO ATTRAVERSO I SUOI SCRITTI. By *Nicoletta Cavalletti*. [Collana del Museo Trentino del Risorgimento e della Lotta per la Libertà.] (Trent: [the Museo.] 1967. Pp. 324.)

THIS book suffers from many serious faults. The most serious is that it is written too much in the style of old, romantic *Risorgimento* history, with the Italian patriots as its heroes and the Germans, of both Austria and the German states at large, as its villains. This gives it an unfortunate smack of immaturity. There are, too, particular as well as general errors of judgment, as, for example, on the characters of and relations between General Radetsky and Victor Emmanuel II. On this subject the author would have done well to read Howard M. Smyth's work. And, though it is no fault of the author, it must be mentioned that toward the end the pagination of my copy of the book is amiss, with some pages printed twice and some missing altogether.

After this, it would seem contradictory to say that the volume in question has some merits, but, on the informative side, it does. Centering on the career of the Abbot Giovanni Prato, it presents him as a fine example of a nineteenth-century liberal Catholic prelate. Born in Trent in 1812, Prato became the leading figure of the Trentino's struggle, first for autonomy within the Austrian Empire, and then for union with Italy. Treated in the course of the book are, on the one hand, his political ideas and activity and, on the other, his religious and social ideas. Dedicated, in the political sphere, to the principle of nationality and to all the liberal ideals of government to which his generation aspired, ideals that he championed through journalistic as well as political activity, Prato was an almost perfect, if minor, representative of the early ninteenth-century liberal national movement. And the high point of his career came in 1848–1849, when he was successively the representative of the Trentino at the Frankfurt Assembly, the Vienna Constituent Assembly, and the Kremsier Assembly, fighting for autonomy for the Trentino, and maintaining, as so many at the time did, that the only way the Austrian Empire could survive was to adopt a federative structure. In the religious and social sphere, on which the author is good, Prato called for reform of the Church from within, maintained that the papacy could only gain by the

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separation of church and state and the end of the temporal power, and recognized very early that the Church had, on the very basis of Christian principles, to espouse the cause of social reform. He was, indeed, as the author shows well, a precursor of Christian socialism.

University of California, Santa Barbara DONALD A. LIMOLI

L'ECONOMIA ITALIANA DAL 1861 AL 1914. Volume I (1861–1894). By *Gino Luzzatto.* [Studi e ricerche di storia economica italiana nell'età del Risorgimento.] (Milan: Banca Commerciale Italiana. 1963. Pp. 291.)

UNTIL his death in 1964 Professor Luzzatto edited this series sponsored by the *Banca Commerciale.* He had devoted many years to the study of the Italian economy between 1860 and 1914. The present volume covers the troubled decades between 1860 and 1894 and was to have been the final monograph in the series. Luzzatto's study is a great work of historical synthesis and will long remain an indispensable guide for all students in the field.

It covers the decades that marked the beginning of Italy's transformation from a peasant economy to an industrial economy. These were years of slow structural change, which saw the development of a national market and the widening of economic and social differences between north and south. Luzzatto focuses broadly on the major problems that faced the Italian economy in this period—financial and monetary troubles, budgetary deficits, currency convertibility, capital shortages, agricultural depression—and follows chronologically the major swings of the Italian business cycle: the economic crisis of the 1860's, the slump of the 1870's, the abortive industrial upswing of the 1880's, and the inflationary boom and crash of 1887–1893 that culminated in the collapse of the Italian banking system in 1894.

Italy, Luzzatto shows, was a backward agricultural economy in the decades after unification. The state and investment banks, in the absence of traditional prerequisites of economic growth—capital, raw materials, technology, and entrepreneurship—played a very large role in Italian industrialization. Luzzatto acknowledges the great contribution of the state to Italy's economic development in these decades through the creation of a national market and of an economic infrastructure, but he is also critical of much government action. He finds that the government's role in fiscal and budgetary matters and in tariff policy led to substantial misallocation of resources. Protective policy in agriculture, he points out, imposed a heavy burden on Italian industrialization. High industrial tariffs in textiles and particularly in iron and steel, in which Italy had a strong comparative disadvantage, acted as a heavy drag on the expansion of newer industrial branches like machine building. Expansionary monetary policy during the inflationary crisis of 1887–1894 led to the collapse of the great investment banks, the *Credito Mobiliare* and the *Banca Generale,* and a large part of Italian industry. The state, in short, may have done more to retard than to accelerate Italian industrialization in these decades.

Luzzatto's study underlines the very fragile foundations of the Italian economy in the decades following political unification and particularly the backwardness of the agricultural sector. The data on agriculture are in sharp con-

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trast with the study on Italian national income (*Indagine sullo sviluppo del reddito nazionale dell'Italia dal 1861 al 1956*) published in 1957 by the Central Institute of Statistics and with the recent work of Rosario Romeo (*Risorgimento e capitalismo* [1959]) on the stages of Italian economic growth and specifically on the role of agriculture in preparing the industrial upsurge. The statistical foundations of Italian economic history in these decades remain, unfortunately, very insecure. Luzzatto has left little doubt, however, as to the backwardness of the Italian economy in these years and of the belatedness of Italy's "big push" which was not to begin until the following two decades.

One hopes that this remarkable work by the late *doyen* of Italian economic historians will be translated into English. It is an important contribution to modern Italian history and a valuable addition to the current literature on comparative economic development. A collection of supporting documents covering the years 1860–1894 is to be published. The manuscript of the second volume, carrying the story to 1914, was, unfortunately, barely begun at the time of his death.

Northern Illinois University IRA A. GLAZIER

L'HISTORIOGRAPHIE DE L'ITALIE CONTEMPORAINE. By *Leo Valiani*. French translation by *Maurice Chevallier*. [Travaux d'histoire éthico-politique, Number 17.] (Geneva: Librairie Droz. 1968. Pp. 170.)

THE precise scope of this useful and thoughtful survey is more readily apparent in the subtitle of the original Italian version, "La storia d'Italia dal 1870 al 1915: Contributi storiografici dell'ultimo ventennio" (*Annali della Fondazione Luigi Einaudi*, I [1967], 35–130). Valiani and nine other Italian professors were asked by the recently formed *Società degli Storici Italiani* to present critical appraisals of the contributions of the past twenty years that Italian specialists have made in the various fields of history. These papers were presented at the society's first national congress held in Perugia on October 9–13, 1967. At the last minute Professor Valiani was also asked to present a critique of studies dealing with the period from 1915 to 1945, but this latter effort has not yet been published.

The author, whose own orientation is that of a democratic socialist, focuses his criticism on seven problems. The first concerns the over-all judgment to be made of liberal Italy between 1870 and 1915. Contrasting Benedetto Croce's speculative and favorable interpretation of that era with the penetrating but sometimes too negative one presented by Gioacchino Volpe, Valiani indicates his own preference for the more empirically researched middle position taken by Federico Chabod.

The second topic is that of the problem of revolution, as set forth in the prison notebooks of the Communist leader, Antonio Gramsci—notebooks whose publication after the war stimulated much discussion among Italian intellectuals. Closely related to that is the debate over the best means of achieving economic development in Italy after 1870. Here the views of Rosario Romeo, Alexander Gerschenkron, and others are discussed. In "The Ruling Class and Men of Government," Valiani comments on recent studies dealing with Crispi and Giolitti and international relations during the Age of Imperialism. The next chap-

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ter, "The Socialist Movement," analyzes postwar studies by Carlo Morandi, Giuliano Procacci, Gaetano Arfé, Aldo Romano, and others. Chapter VI, entitled "The Catholic Movement," takes up such researchers as Fausto Fonzi, Gabriele De Rosa, Giovanni Spadolini, Pietro Scoppola, and Ettore Passerin d'Entrèves. Finally, in a brief discussion of "The Nationalist Movement," Valiani notes the contributions of Nino Valeri, Raffaele Molinelli, and Franco Gaeta.

In a short concluding chapter, the author takes Italian historians to task on a number of points. Though he concedes that they can all be proud of the world-wide attention accorded to Croce's historical thought, Italians ought to consider why their scholars have not produced any major historiographical debate since the war. For example, he notes, there has been nothing in Italy comparable to the controversies regarding the two world wars that have stirred Anglo-American and German scholars. And whereas numerous foreigners during the past two decades (Christopher Seton-Watson, Denis Mack Smith, John Thayer, Maurice Vaussard, and others) have written significant broad interpretations of the 1870–1915 period of Italian history, almost no natives except Giacomo Perticone and Ivanoe Bonomi have endeavored to do so since the time of Croce and Volpe. Moreover, Italian scholars have largely ignored the history of other parts of the world—apart from such noteworthy exceptions as Franco Venturi's study of Russian Populism, Valiani's own account of the dissolution of the Habsburg Empire, and certain studies of French history by Aldo Garosci, Luigi Salvatorelli, and Gastone Manacorda. Finally, Valiani asks his fellow countrymen the pertinent question: why are they so reluctant to write biographies and to embark upon quantitative studies in the field of socioeconomic history?

Vanderbilt University CHARLES F. DELZELL

THE ITALIAN ANTI-FASCIST PRESS (1919–1945): FROM THE LEGAL OPPOSITION PRESS TO THE UNDERGROUND NEWSPAPERS OF WORLD WAR II. By *Frank Rosengarten.* (Cleveland: Press of Case Western Reserve University. 1968. Pp. xx, 263. $6.95.)

PROFESSOR Rosengarten, a member of the Italian department at Queens College, has provided us with a useful account of the scope, activities, and substantive political discussions of the Italian anti-Fascist press. While this volume turns up nothing startlingly new, it is, nonetheless, a valuable companion piece to Delzell's *Mussolini's Enemies.*

The author illustrates the intellectual vitality of the resistance forces in Italy as well as the variety of political perspectives contained within the movement. These perspectives emerge clearly after the liberation and become the basis for the continuing political debate in post-Fascist Italy. Therefore, those who are primarily interested in contemporary Italian politics will also find the volume useful.

One of the best chapters in the book concerns the "anatomy of fascism," or the various interpretations of the nature and genesis of fascism offered by Communist, socialist, actionist, liberal, republican, and Christian Democratic journalists. A key problem attracting their attention was whether fascism should be seen as a "parenthesis" in Italian life, that is, an aberration organically un-

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related to the previous liberal order, or whether it was an outgrowth of contradictions in that order itself. None of the parties engaged in the resistance struggle seems to have adopted a unicausal explanation of this complex phenomenon.

The author brings out well the tactical astuteness of Togliatti and his grasp of the specific contours of the Italian political situation. The extent of the subsequent electoral success of the PCI becomes more comprehensible in the light of the role of the party in the resistance and the exceptional ability of its leadership.

Rosengarten properly gives emphasis to the contribution of the Action party, despite its rather scant following and subsequent demise, because of the intellectual quality of its contribution to the resistance. He might well have included reference to Guido Dorso, who earlier had written for Gobetti and who edited the party's southern newspaper from 1943 to 1945. Dorso's analyses of Fascism and the structure of Italian society are noteworthy. The book could also profit from a concluding chapter.

University of Virginia DANTE GERMINO

A SHORT HISTORY OF MODERN GREECE. By *C. M. Woodhouse.* [Praeger Short Histories.] (New York: Frederick A. Praeger. 1968. Pp. 318. $6.50.)

IN this, his fourth distinguished contribution to Greek historiography, C. M. Woodhouse has successfully met the need for a brief, comprehensive, up-to-date history of modern Greece in English. This account, essentially an introduction for the student or general reader, starts with the foundation of Constantinople in 324 and carries the story down to the end of 1967. It displays the strengths that one has come to expect of its author: clear and engaging writing, judicious evaluation of still-controversial developments, the right balance between facts and generalizations. It supersedes earlier English-language one-volume histories of modern Greece and from now on is clearly the place for anyone to begin. But for bibliography, on which it is inadequate, and for coverage of the economic, social, and cultural, which is treated largely as an adjunct to a basically politico-diplomatic focus, this book should be supplemented by the excellent Greek chapters in Leften Stavrianos' *The Balkans since 1453* and, if one reads French, by Nicolas Svoronos' *Histoire de la Grèce moderne.*

The book contains occasional errors of fact. The Ottoman capitation tax was *cizye,* not *kharaj.* The Ottoman Turks did not capture Tripoli, Acre, and Jerusalem at the end of the thirteenth century. The Bavarian regency did not enjoy a fourth year, its term being only for two and a half years. Armansperg did not retain Mavrokordatos in Greece in 1835; the latter had been sent abroad in June 1834. Rudhart's resignation resulted not from British insistence but from a clash with Otho himself. Occasionally the author's judgments are questionable. To say that the Greeks "might even have taken over the Ottoman Empire as a going concern" is to exaggerate their influence in Ottoman administration. In calling revolutionary leaders "only nominally Greek," the author is contradicting his own original definition of "Greek." Finally, in dealing with the Byzantine and Ottoman periods, the author presents largely a general survey of these two empires rather than a precise picture of Greek ethnic development within them, as he

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might have done by utilizing Vakalopoulos' impressive two-volume study of "modern Hellenism" (1204–1669) in Greek. It is misleading to treat Byzantium, from the Age of Constantine, as if it were a part of "modern" Greek history. A "modern" national consciousness is discernible only at the end of the Byzantine period. The space devoted to the pre-1204 period might have been better used to show more directly what Byzantium contributed to Greek modernity and to discuss the role of the ancient Greek heritage during the Byzantine and Ottoman periods.

Amherst College JOHN A. PETROPULOS

WOJNA WYZWOLEŃCZA NARODU POLSKIEGO W LATACH 1939–1945 [The Liberation War of the Polish People 1939–1945]. Volume I, WEZŁOWE PROBLEMY [The Key Problems]; Volume II, SZKICE I SCHEMATY [Maps and Charts]. By *Tadeusz Rawski et al.* Edited by *Ernest Wiśniewski.* [Wojskowy Instytut Historyczny.] (2d ed.; Warsaw: Wydawnictwo Ministerstwa Obrony Narodowej. 1966. Pp. 881, 80 plates. Zł. 120 the set.)

THE first edition of this work appeared in 1963, but the general approach is still the same. It can be summarized in the foreword by Marshal Marian Spychalski: "The science of history should serve, and does serve, the practice of the present. It serves the new, progressive social classes, since they, above all, can fully benefit from its study in order to clear the way for the progress in which they have the most vital interest." Writing of the educational value of military history, he says: "For us, the most valuable traditions, from the point of view of their educational content, are the traditions of the antifascist, revolutionary war of national liberation of 1939–1945. These are the traditions of the struggle for a new polity, for a socialist Poland, for the attainment of a politically correct ideology, for the right action and the right attitude. . . . We hope that this study will contribute to a better and deeper understanding of these traditions, and will clear away the distortions."

Guided by these criteria, the authors have produced a history of the Polish struggle in the Second World War, which gives pride of place to the Communist "People's Guard," renamed "People's Army" in 1944, and to the Polish military units formed in the USSR. There is very little on the non-Communist "Home Army" and the Polish armed forces in the West, and what is there is presented in terms of political condemnation. The chief asset of the book is that those interested in the military side of the Communist movement in Poland from 1942 to 1945 will find every partisan action described in detail, as well as information on the formation and training of the Kościuszko Division and other units in the USSR. This virtue cannot, however, make up for the complete distortion achieved by presenting the Communist underground in Poland and the Polish units in the Soviet armed forces as the dominant factors in the Polish armed struggle during the Second World War.

The "Home Army" numbered 360,000 men in August 1944, and the non-Communist but independent "Peasant Battalions," 150,000. Compared with these figures, the "People's Army" numbered only 25,000 in August 1944. The Polish

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armed forces in the West in 1943–1944 numbered about 150,000, while Polish units in the Soviet armed forces stood at about 15,000 until they were massively reinforced by conscription in Poland in 1944–1945. It is ridiculous to claim that the "People's Guard" initiated armed resistance to the German occupation, when it was only organized in 1942, while non-Communist resistance was active long before that time. In fact, the "People's Guard" followed the example of the ZWZ, later renamed the "Home Army," in organizing small units for limited terrorist action and sabotage against the Germans. This fact did not prevent Communist propaganda at the time from accusing the "Home Army" of passivity and even cooperation with the Germans, a theme that is also present in this work.

In order to obtain a true picture of the Polish armed contribution in the Second World War, the reader will still have to rely on *Polskie Siły Zbrojne w Drugiej Wojnie Światowej* [The Polish Armed Forces in the Second World War] (London, 1950–62). Though the authors consistently accuse this work of distortion, they pay it the compliment of frequent reference in their book. It is regrettable, though hardly avoidable, that all works on the Second World War published in Poland are written to prove that the only correct policy was Polish cooperation with the USSR. At the same time, these studies present Polish Communist military action as the mass movement, which it never was, in order to provide a historical justification for the Communist government of postwar Poland.

University of Kansas ANNA M. CIENCIALA

MEMOIRS OF A REVOLUTIONARY. By *Eva Broido*. Translated and edited by *Vera Broido*. (New York: Oxford University Press. 1967. Pp. xii, 150. $4.00.)

EVA Broido was an active participant in the 1917 February Revolution in Russia and rose to assume the position of secretary-general of the Menshevik Central Committee. After the Bolshevik triumph, she and her husband, Mark Broido, left Russia and settled in Berlin. It was there that Eva Broido wrote the memoirs that her daughter, Vera Broido, has so admirably edited and translated some forty years later. In a simple and unemotional manner, *Memoirs of a Revolutionary* presents a chapter in the life and work of Russian revolutionary activists between 1898 and 1917. Eva Broido shows how these courageous men and women were not content to deal in theory alone, but went and lived among the workers to educate them, organize them, and suffer with them. The devotion of people such as Eva Broido was extraordinary. In spite of imprisonment, exile to Siberia, and separation from her children, she returned again and again to her work to establish democratic socialism. Twice she left the comforts of foreign exile to return to Russia. Ironically the second time was to fight Bolshevik tyranny. Eventually she paid with her life, for Eva Broido was probably murdered in the massacres of June 1941, when Stalin ordered all Mensheviks to be shot.

Born of Jewish parents and raised in the limited environment of the Russian Pale, Eva Broido managed to receive a university education through sheer will. In 1896, at the age of twenty, she went to Berlin and experienced what can only be described as a "conversion" to socialism: "When I left Berlin three months

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later I was a fervent and convinced socialist." Such brevity is typical, for throughout the memoirs there are almost no self-analysis and no awareness of possible unconscious motives. On the other hand, Eva Broido does not present a simplified view of political and social reality. She is never sentimental; she had a genuine respect for the working class, but she never foolishly eulogized its virtues.

The translation of Eva Broido's memoirs appears to be of little "scholarly" interest. They have long been available in an authorized German edition, although Vera Broido worked from her mother's original manuscript and included extra notes that she found there. In the memoirs themselves there are no discussion of theory and almost no real content concerning the conflict between Mensheviks and Bolsheviks. Even the account of the *Romanovka* protest in Yakutsk in 1904 is from her husband's writings. There is no description of the events of 1917 since the memoirs end with the beginnings of the Revolution. Yet this very personal document is of value to the undergraduate and the general reader seeking to understand the revolutionary temperament and way of life in Russia at the turn of the century. *Memoirs of a Revolutionary* is a moving *document humaine* of interest to all who follow the progress of the human spirit.

Rutgers University, New Brunswick, New Jersey HAROLD POOR

DIE KRAFT RUSSLANDS: WIE BEURTEILTE DIE POLITISCHE UND MILITÄRISCHE FÜHRUNG DER EUROPÄISCHEN GROSSMÄCHTE IN DER ZEIT VON 1905 BIS 1914 DIE KRAFT RUSSLANDS? By *Risto Ropponen*. [Historiallisia Tutkimuksia, Number 74.] (Helsinki: Suomen Historiallinen Seura. 1968. Pp. 321.)

FEW tasks are more important for leaders of state than the peacetime evaluation of the strength of potential allies and enemies. In the event of war, the success and possibly the very existence of a state may depend on how correct such evaluations have been. Attempts to estimate the strength of another power should therefore be fruitful fields of historical investigation, and none is more so than the one selected by Dr. Ropponen: Russia in the years between its defeat by Japan and World War I. Unfortunately, the results in this case are disappointing.

Ropponen has examined an impressive number of published and unpublished sources, but he has failed to keep in mind the main object of his investigations. Instead of focusing on Great Power estimates of Russian strength and the effect of such estimates on the policies of these powers, he has interpreted "strength" in the broadest possible way and written a survey of Russian government and society as seen by foreign observers. Their analyses of the minorities problem, the economy, the church, the personality of the Tsar, and similar subjects are interesting enough, but not enough new information or additional insight is introduced to warrant such lengthy treatment. Not until his final chapter on foreign opinions about the actual military strength of Russia does the author come to grips with his subject, and even here his findings are scarcely novel. It is no surprise to learn, for instance, that respect for Russia's military strength declined sharply as the result of defeat and revolution in 1905, or that

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there was a revival of belief in Russian power shortly before 1914, especially in Britain and France. More interesting is Ropponen's evidence showing to what extent foreign estimates of Russian strength were based on wishful thinking. Whereas most German and Austrian leaders believed Russia would not risk a foreign war for fear of domestic revolution, their British and French counterparts were not only confident that Russia would fight but that victory over the Teutonic powers would strengthen the tsarist government. Had Ropponen concentrated on this theme, he would, in my opinion, have written a more valuable book. As it is, one can only agree with his conclusion that estimating the strength of another power is a very difficult business.

Brown University NORMAN RICH

EXPANSION AND COEXISTENCE: THE HISTORY OF SOVIET FOREIGN POLICY, 1917–67. By *Adam B. Ulam.* (New York: Frederick A. Praeger. 1968. Pp. viii, 775. $12.95.)

FOR years two outstanding texts have dominated the field of Soviet foreign policy: Louis Fischer's *The Soviets in World Affairs, 1917–1929,* and Max Beloff's two-volume *The Foreign Policy of Soviet Russia, 1929–1941.* Nothing comparable in quality and scope, quite apart from specialized monographs, has been published for the period after 1941, although Herbert Feis's and William H. McNeill's diplomatic histories of World War II were rich sources of information on Soviet policy. In addition, a very useful single-volume history appeared in 1963: Robert D. Warth's *Soviet Russia in World Affairs* surveyed Soviet foreign relations up to the 1960's.

The publication in 1968 of two volumes with strikingly different approaches, *Soviet Foreign Policy,* by Jan F. Triska and David Finley, and *Expansion and Coexistence,* by Ulam, changes the picture and sets it at a new angle. The first of these two works represents the first major attempt to apply new techniques of political science to Soviet foreign policy over the fifty-year span.

The authors take as a starting point the existence of many excellent histories concerning Soviet activities in world affairs and address themselves instead to the "how" and "why" questions, to a methodical examination of divergent interpretations that employs a variety of modes of analysis to do so. Thus, for example, they use quantitative content analysis to examine the role played by Marxist-Leninist doctrine in the Soviet foreign policy process and multiple symmetry models to explain Soviet relations with the West. Whether their model of Soviet behavior adds a new dimension to analysis I leave to the reader, since my charge is the Ulam book, but coincidental publication of these volumes provides an unusual opportunity to compare the merits of the "traditional" and "behavioral" approaches.

The Ulam volume, "traditional" though it is, is an enviable achievement. The author has compressed—the word is used advisedly in spite of the book's length—fifty years of events into an engagingly written, highly analytical political history. What could well have been a surface recounting of events takes on depth through sophisticated interrelationship of the domestic and foreign factors that went into the making of policy. Roughly equal space is allotted to the pre-

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and post-World War II periods. The bridge between them is provided by an examination of Soviet policy during the war in which Stalin's reputation as a diplomatist is at least partially rehabilitated. From here, Ulam launches into a discussion of the cold war which he views largely as a product of a totalitarian society inexorably seeking security through constant expansion. Prevention of West German rearmament becomes a principal aim of Soviet policy. "The Perils of Khrushchev" (Pauline?) are in no small part attributed to the Chinese defection and the fragmentation of the Communist movement. Soviet policy in Vietnam, the Middle East, and elsewhere carries the narrative to the present.

This sketchy topography cannot do justice either to the scope of the author's coverage or to his suggestive ideas and intriguing formulations. Nor does limited review space permit extended critical examination; a few comments will have to suffice.

The scope of the book is so sweeping that a bibliography, preferably annotated, would have been most useful. Failing this, the reader would have profited from more extensive and explicit footnoting. The rendering of Russian sources in English without also giving the Russian is irritating and perhaps misleading. A few maps would have been useful, not because I am without an atlas but because the book seems destined to become required reading for students.

The title of the book is misleading. This criticism is not formal, but rather raises questions concerning the author's central propositions. By expansion, and I trust I have it straight, Ulam does not mean merely the accretion of Soviet territory. Expansion also subsumes areas that fall to Communism under the direct or indirect control of the USSR. That the Chinese Communist seizure of power added to Soviet post-World War II expansionism is an interpretation with which some might have taken issue, even in 1949. Nevertheless, if it is conceded for the sake of argument that the establishment in China of Communism represented a form of Soviet expansionism, given Stalin's hold over the world Communist movement and Chinese dependence on Moscow, the situation has changed drastically. On the evidence of the Sino-Soviet break presented in the book itself, the title would have been rendered more accurately by "Expansion and Contraction," or perhaps by "Expansion, Contraction, and Coexistence." Moreover, the author's account of Soviet policy, as distinguished from his characterization of it, strangely does not justify the idea of expansionism as endemic to the Soviet system. The description of Soviet acquisition of territory appears generally in the context of strategic policy. Thus, the author's forceful and convincing interpretation of the diplomatic maneuvering that left East Central Europe in the Soviet sphere of influence demonstrates geopolitics in action rather than the expansionist dynamic of a totalitarian system.

In his concluding paragraph, the author suggests that the Soviet leaders are faced with a choice of pursuing the phantom of Communist unity ("unattainable") and ideological expansion which "clashes with her national interest" (which has been the case for years and has consequently been subordinated to *raison d'état,* as is so evident from Ulam's study). But, the author comments, there is another approach to international affairs in the nuclear age, which would forsake expansionism in order to seek world stability. "The impulse to continuous expansion, the goal of world hegemony clash, now much more than

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then, with that vision of a world order without which no state, no matter how powerful, can find real security and true greatness." If Ulam's admonition is meant to be applicable to American as well as Soviet forms of expansion and hegemonic policy, let me conclude with an "Amen."

Indiana University BERNARD S. MORRIS

Near East

ISLAMIC HISTORY OF THE MIDDLE EAST: BACKGROUNDS, DEVELOPMENT, AND FALL OF THE ARAB EMPIRE. By *Wilson B. Bishai.* (Boston: Allyn and Bacon. 1968. Pp. xvi, 399. $7.95.)

THE political history of the Arab caliphates during the golden age is the focus of this textbook. A narrative of events from Mohammed through al-Mutawakkil fills nearly two-thirds of the text. The remainder consists of three sections of approximately equal length. The opening section briefly surveys geography and the ancient Near East. The last two sections deal with the regional dynasties from the middle of the ninth to the middle of the thirteenth century and cover government, religion, learning, and society.

The book passes the all-important test of accuracy. So far as I can judge, there are no major errors. In addition, the book is remarkably free of typographical slips. A few features, however, are likely to cause difficulty for the nonspecialist, student, or teacher. The author treats the Qays-Yaman conflict in such a way that the uninitiated will surely conceive of them as "clans" within some enormous Umayyad "tribe" rather than as two factions, each attributing itself a common ancestry, which encompassed and dichotomized all the Arab tribes within the Umayyad territories. Some of the book's intended readers are also likely to be confused by the names that the author uses in referring to some of the leading personae. Most notably, he employs only Ibn-al-As and Ibn-al-Zubayr without giving, as is usual in textbooks, the personal names Amr and Abdullah. The names used for the Shiite Imams are likely to cause similar difficulty.

Bishai's emphasis and general coverage are quite similar to those of other general works, but in dealing with particular reigns and events he often provides detail missing from other texts, and he writes with an emphasis all his own. On occasion, his emphases strike me as peculiar. One such case is the relatively brief treatment of Muawiyah I. Also perplexing is a relation of Mukhtar's career without a single reference to the non-Arab converts. In general, however, Bishai's account is balanced; his selection of detail, discerning and judicious.

A judgment of intelligibility to students is always hazardous. I had two reservations. The presentation of the regional dynasties is likely to confuse the student about some points, and the chapters on Islamic sects and learning are too brief to be meaningful.

University of Illinois C. ERNEST DAWN

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Africa

THE FORTUNATE SLAVE: AN ILLUSTRATION OF AFRICAN SLAVERY IN THE EARLY EIGHTEENTH CENTURY. By *Douglas Grant.* (New York: Oxford University Press. 1968. Pp. xii, 231. $7.50.)

In 1731, Ayuba Suleiman Diallo, a Fula marabout from Bondou in eastern Senegal, was captured while on a trading expedition and sold into slavery in Maryland. Fortunately for Diallo, a letter written in Arabic and addressed to his family in Bondou fell into the hands of James Oglethorpe and led to his being brought to England where he was entertained royally. In 1734, the Royal African Company sent him back to Bondou, where it hoped to use him in its rivalry with the French establishments on the Senegal River.

Douglas Grant has written an entertaining account of Diallo's story, supplementing the sparse documents with chapters on the slave trade, Senegambian ethnography, English attitudes to Africa and slavery, and the Anglo-French competition in Senegambia. Certain chapters are excellent, especially the one on Diallo's redemption and stay in England. It is unfortunate that Grant knows England better than Africa. Though he has dug further in the quest for documents than Philip Curtin, who edited Thomas Bluett's version of Diallo's reminiscences in his *Africa Remembered,* Grant, lacking Curtin's extensive knowledge of the history and ethnography of the area, has relied primarily on travel accounts of the period. The result is a succession of errors—some petty, some important. No knowledgeable scholar could speak of the Gambia having "hardly changed in two hundred and fifty years." Futo Toro was never anyone's "fief" and had long since ceased to be dominated by Djoloff, and it is difficult to speak of "the Muslim's jaundiced view of the African," when those Muslims are Africans. Frequently, Grant simply repeats the prejudices and preconceptions of his sources, as, for example, in talking about the characteristics of the Fula.

For the scholar, the book adds little to Curtin's skillful editing of its most important source. Grant does, however, tell the story well, and the story is one of the few to provide brief glimpses of African history from an African point of view.

University of California, Berkeley Martin Klein

SOUTH WEST AFRICA AND ITS HUMAN ISSUES. By *John H. Wellington.* (New York: Oxford University Press. 1967. Pp. xxiv, 461, 12 plates. $13.45.)

Students of African affairs have long wanted a full-scale history of South-West Africa. We had Vedder, *South-West Africa in Early Times . . . up to 1890; The Native Tribes of South West Africa . . .* provided an ethnographic survey; Goldblatt, *The Mandated Territory of South West Africa . . .*, covered the period of the League (1919–1939) and beyond to 1949; then numerous polemical accounts against governance by South Africa, as, for example, Lowenstein, *Brutal Man-*

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date . . ., or First, *South West Africa,* and some impartial but narrow studies like that of Bruwer, *Southwest Africa: The Disputed Land*. Now Professor Wellington offers us his book. It is good; it is useful; it is fuller than any other; but it still leaves us without an adequate history of South-West Africa.

Wellington knows his subject; he has studied the area for over forty years. He attempts to place the human problem of the mandate in three perspectives: geographical, historical, and political. While he succeeds in his first goal, he fails partially but seriously to give us the historical and political dimensions of this troubled land. Most damaging to the value of his book is its failure to bring alive the human element in the drama—in spite of the title *South West Africa and Its Human Issues*.

In the first part of the book the physical frame, climate and vegetation, and economic development are described with great detail and authority. Part II treats the indigenous peoples of the area and the history of German colonization. Here is a clear account of the early history of the bewildering variety of peoples of the territory, though Wellington does fall victim to old-fashioned ethnography by suggesting that the Hottentots and Herero had a Northeast African origin and were a mixture of Hamitic-Bantu peoples. As Murdock has shown, this is not true. The Hottentots, for example, are simply Bushmen who adopted cattle culture. The cruelty and arbitrary nature of early German rule is recounted in sad detail: cheating, sexual exploitation, land theft, and finally Von Trotha's famous extermination edict. This story reads like an account of the decimation and mistreatment of the American Indian. Part III discusses the mandate period, the United Nations, changing patterns of race relations around the world, apartheid, and the legal struggle over South-West Africa.

Many chapters are valuable and informative. The section on the World Court's judgment (1966) is excellent. But the historical and political treatment after 1919 is less full and less satisfactory. Almost nothing is said about the nonwhite peoples of the territory. They become simply objects administered by whites or fought over in international courts. Still, his conclusion is worth reporting: the record is clear that South Africa has failed in its sacred trust to develop the people of the mandate. The interests of the whites have been served more than that of the blacks.

There are some errors in spelling of names, and some chronology has gone wrong (Leutwein could not have met the Herero chiefs in January of 1896 because he had left by then; the Congo Free State was annexed in 1908, not 1909), but these are minor and inevitable errors in such a large and complicated book. Wellington has given us more than anyone else. Even if it is not all we might have wished for, we are still grateful.

Hoover Institution PETER DUIGNAN

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Asia and the East

ASIAN FRONTIERS: STUDIES IN A CONTINUING PROBLEM. By *Alastair Lamb*. (New York: Frederick A. Praeger. 1968. Pp. ix, 246. $5.00.)

SEARCH FOR NEW GUINEA'S BOUNDARIES: FROM TORRES STRAIT TO THE PACIFIC. By *Paul W. van der Veur*. (Canberra: Australian National University Press; distrib. by Martinus Nijhoff, The Hague. 1966. Pp. xii, 176. 25.50 gls.)

THESE volumes enhance our understanding of border problems in Asia. Both authors have made contributions: Lamb by developing a synoptic analysis of Asia's frontiers and the diplomatic strategies that led up to the present situations; Van der Veur by creating detailed chronological essays about the development of New Guinea's boundaries. The particular merit of *Asian Frontiers* is its judicious blend of political history and geography. The analytic framework is strengthened at the outset by stating the peculiar geopolitical problem left by colonial power struggles between China, Russia, France, and the United Kingdom. This method is pursued by constant adherence to the goals in foreign policy that caused the four great Asian powers of the colonial era to shift tactics and alliances through time and to press into or withdraw from buffer areas.

This book covers immense territory and occasionally generalizes about specific issues. The expert will be annoyed by some of Professor Lamb's gross statements, but the newcomer to Asia, the diplomatic historian, the policy maker, and the teacher of geopolitics will all be rewarded in reading this book. It is the first of its kind to deal with the policy implications posed for the Great Powers as they struggled with problems inherited in the borders around China. Another strength is the author's sensitivity to ethnic and linguistic consciousness throughout Asia; this factor causes many existing state boundaries to seem artificial. Chapters on NEFA, India's northwestern frontier, Central Asia, and the Sino-Soviet frontier are especially astute and reflect Lamb's earlier research interests. Conversely, the abbreviated discussion of borders in Laos, Cambodia, and the Vietnams will add little to the wisdom of those who follow the war and read the usual works on French Indochina.

Professor van der Veur's study of New Guinea is an expert's reference book. Significant sources from Dutch, German, British, Australian, and Indonesian authorities are consulted and used with great objectivity. I could find no debilitating bias on the part of the author, other than a healthy disrespect for these governments that have allowed most of New Guinea's boundaries to go unmarked despite a century of diplomatic negotiation. Admittedly the problem is without parallel because of the terrain, climate, and occasional head-hunters, yet the meticulous research that undergirds this study reveals an amazing lack of interest in financing the boundary demarcation. The sanguine attitudes of all the governments illustrate a common illusion among policy makers: if a problem has not been defined so as to enter an in-box, then forget it until a foundation finances a review commission or until a riot or a civil war erupts. It is to be hoped that this thorough review of the diplomacy and the stakes involved will motivate the

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formation and financing of a joint Indonesian and Australian team, sanctioned by the UN, to complete a task begun by the Dutch in 1848.

Van der Veur's study has moments of wry humor, but its prose is marked by some turgidness and disorganization. It is not a book for the casual student; nor is it likely to fit in most Asian bibliographies; it is, however, the most authoritative statement in print about the New Guinea border problem. No writer on New Guinea history or politics can afford to ignore this scholarly contribution.

School of Advanced International Studies, Johns Hopkins University JOHN H. BADGLEY

LES ORIGINES DE LA RÉVOLUTION CHINOISE, 1915–1949. By *Lucien Bianco*. [Collection Idées.] ([Paris:] Gallimard. 1967. Pp. 384.)

IF even a so-called expert on modern China contemplates the difficulties of writing a general history of the Chinese Revolution, he will understand why no such satisfactory survey has yet been written. However, specialized, monographic studies have progressed far enough, and the needs of lay and student readers have become sufficiently acute, that some attempts to supply this lack have been initiated.

Lucien Bianco evidently decided not to attempt a general and fairly thorough treatment of the currents and flow of Chinese history through the first half of this century. Rather, he sketches backgrounds and revolutionary periods and then poses for each later period the really big questions implied by his title: the search for sources of that revolution—why and how it culminated as it did in 1949. He is also interested in comparing, and contrasting, Chinese Communist experience with the doctrines of Marxism-Leninism and with Soviet experience, and, toward the end of the book, with varied patterns evolving in the third world. Consequently some full chapters and the latter half of others are written in a stimulating essay vein.

There are certain hazards in the use of this approach, and this work has not emerged entirely unscathed. There is the problem of providing the less sophisticated reader with so little factual history that he must acquire it from other sources before he can judge the soundness of Bianco's analytic interpretations. Bianco does, however, summarize conditions and trends that cogently relate to his conclusions. A tone of inevitability, based on the advantages of hindsight and on analysis of causal forces, gathers momentum throughout the work. Occasionally one needs to ask whether there were alternative lines of development and why they did not succeed. Such questions are not wholly ignored here; the fifth chapter ("Reform or Revolution?") appraises almost a quarter of a century of Kuomintang efforts toward national unity, economic growth, and institutional change, along with reasons why they proved inadequate.

Historical sketching, particularly in the introductory survey, was allowed to preclude asking some of the central questions about earlier revolutionary periods in a manner afforded in periods after 1927. For example, what of fundamental significance was happening in China during the period when warlordism was rampant? And if one viewed that chaos as a Chinese nationalist might have at that time (possessing, in addition, the advantage of hindsight and of comparative

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revolutionary experience), what might one say about the requisites of national unification and the overcoming of centrifugal interests? This is important because it involves those elements in the Nationalist phase of the revolution that mainly, though only partially, overcame warlord and regional particularism. And, in so far as the country remained divided, it involves what would still be required to complete the process. This latter part is rather well interpreted in this book.

The influences on Chinese events and trends exerted by foreign powers having interests in China receive only marginal and general attention in a book that is devoted overwhelmingly to internal revolutionary dynamics. But, if we recall Dean Acheson's rueful admission in the White Paper of 1949—that the United States had tried but failed to alter the course of a revolution that resulted as it had largely in response to internal forces—we should admit that the author's emphasis was right.

Of course, it is interesting, particularly in the use of comparative references, to have interpretations of the Chinese transition by a legatee of the French Revolution, empires, and republics. Perhaps also in part because many French intellectuals have learned to live in an environment that includes but does not capitulate to Communism, Bianco could write without the self-defensiveness or cold war biases sometimes observed elsewhere. And yet an American can feel some satisfaction that this work is based largely on studies at Columbia and Harvard Universities and on books, articles, and theses written overwhelmingly in the United States. The French have produced some famous Sinologues who were chiefly interested in premodern periods of Chinese history; it is encouraging to see a few more of their scholars turning to the study of modern and contemporary China.

Fletcher School of Law and Diplomacy ALLAN B. COLE

PARTY RIVALRY AND POLITICAL CHANGE IN TAISHŌ JAPAN. By *Peter Duus.* [Harvard East Asian Series, Number 35.] (Cambridge, Mass.: Harvard University Press. 1968. Pp. viii, 317. $6.95.)

THE history of Japan in the first quarter of the present century is finally receiving the attention it deserves from historians in both Japan and the United States, and Professor Duus' volume is an example of this new interest. It examines developments in Diet politics from 1912 to 1927. In broad terms, this work is a detailed study of two general trends: first, the growth of the strength of the two "established" parties and of their respective leaders, despite strong institutional and traditional obstacles; second, the manner in which this party strength in the House of Representatives covertly controlled seven transcendental or bureaucratic cabinets and caused the elder statesmen, who theoretically had complete freedom to choose a Prime Minister, to select party leaders to head the five other cabinets in this fifteen-year period.

The author considers the Movement for Constitutional Government (1912–1913) as the source of two-party politics and of the "established" parties. The rivalry and occasional cooperation between the strong Seiyukai party, under the astute leadership of the politician Hara Kei, and the newly formed Dōshikai, which developed later into the Kenseikai under Katō Kōmei, started at that time.

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The book then describes the next six years of bureaucratic or transcendental cabinets with the parties exerting their influence indirectly. In 1918, Hara Kei of the Seiyukai was selected to form Japan's first real party cabinet. As the author quite appropriately points out, however, Hara was more interested in a positive policy of internal economic development that would strengthen local bases of his party than in fundamental political reforms such as universal suffrage. After his assassination in 1921, the parties fell into disarray. Following the selection of two admirals and a privy councilor as Prime Ministers, the two parties were forced to join in common cause against the government in the hope of forcing a return to party cabinets. The account then takes up the appointment, in 1924, of Katō Kōmei as Prime Minister. He advocated a policy of retrenchment at home and successfully championed a universal suffrage bill. But scandals in both parties and the simultaneous passage of the Peace Preservation Law to control any subversive activity alienated the general public and discredited the parties. The author notes that the "established" parties had developed when Japan faced no great international crisis and when no other group had the strength and the will to challenge them. He concludes that as there was no guarantee that these conditions would continue the future of party cabinets was precarious.

The author has covered his chosen topic thoroughly and well. He has based his study on extensive Japanese sources and produced a significant and important addition to our knowledge and understanding of a complex period of Japan's political history. It is unfortunate that at times the style and order of presentation of material seem to add to this complexity. Duus' book will become much more understandable for the general reader if he first reads Professor Tetsuo Najita's *Hara Kei in the Politics of Compromise, 1905–1915* (1967). As these two works are clearly complementary, cover some of the same period, and are part of the same "East Asian Series," Duus might well have referred more frequently to Najita's valuable study. Together, these two volumes take us a long way toward understanding Japan's political development in the first quarter of this century.

Columbia University HUGH BORTON

RANA POLITY IN NEPAL: ORIGIN AND GROWTH. By *Satish Kumar*. [Issued under the auspices of the Indian School of International Studies.] (New York: Asia Publishing House; distrib. by Taplinger Publishing Company, New York. 1967. Pp. 195. $4.25.)

THIS is a respectable monograph on a very limited subject that will primarily interest only a small circle of specialists. Because of its geographical isolation, Nepal remained outside the mainstream of modernization during the days of British rule in India. Lethal factional disputes kept the little mountain kingdom in turmoil until a strong, intelligent man was able to massacre his enemies and seize control of the government. This man was Jang Bahadur who became Prime Minister in 1846 and established an oligarchy of the Rana family that ruled from behind the throne until 1951.

Although Jang Bahadur's rule was an improvement over the chaos immediately preceding it, Rana dictatorship rapidly degenerated into a stultifying totali-

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tarianism. The king was reduced to being simply a nominal head of state who performed ceremonial functions. The Rana Prime Minister was actually sovereign of two Nepalese provinces and the supreme legislative and judicial as well as executive authority in the kingdom. Brahmans continued to own tax-free land, to receive charitable income, and to be exempt from the death penalty. The Ranas themselves had originally belonged to an inferior caste, but declared themselves Thakurs, a high order of Kshatriyas, to justify the power that they usurped. To the Ranas' credit, they ran a relatively efficient administration through a system of nepotism. (If Nepal had had trains, they would have run on time!) Members of the Rana oligarchy, like the Mogul *mansabdārs,* were shifted at need from civilian to military duties. Perhaps their most significant contribution was the codification of Nepalese law and the expansion of the court system. But under this tyranny economic and social life suffered grievously, and the history of Nepal since 1951 has been an attempt to recover from the effects of Rana dictatorship with external help.

Such is the main outline of Dr. Kumar's study. But he glosses over or omits some tantalizing questions. What, for example, was the influence of the British residents on Rana polity? Did they do anything more than simply encourage trade relations with India? What effect, if any, did the remittances of Gurkha soldiers serving in the Indian Army have on the economy over which the Ranas presided? It is fair to say that this discussion lacks flare and imagination, but nonetheless large libraries would do well to add it to their collections as an authoritative statement. Kumar has canvassed a rather wide selection of sources, including some Nepali manuscripts at Katmandu. It is too bad that the publishers did not see fit to furnish a more substantial binding.

Los Angeles Valley College MARK NAIDIS

MODERATES AND EXTREMISTS IN THE INDIAN NATIONALIST MOVEMENT, 1883–1920: WITH SPECIAL REFERENCE TO SURENDRANATH BANERJEA AND LAJPAT RAI. By *Daniel Argov.* (New York: Asia Publishing House; distrib. by Taplinger Publishing Company, New York. 1967. Pp. xiv, 246. $6.50.)

DR. Daniel Argov, a historian at the Hebrew University of Jerusalem, has written an able account of the struggle between moderates and extremists within the Indian nationalist movement. As his method for analyzing the conflicting approaches of these two groups, he has chosen to focus on two of its most prominent leaders, Surendranath Banerjea and Lala Lajpat Rai. Banerjea, a Bengali Brahman, was educated in an English college in India and then in London, where he successfully competed in the examinations for the Indian civil service. He was later dismissed—unfairly, he claimed—from the ICS on charges of "gross carelessness and dishonesty" and spent the remainder of his lengthy career as an organizer of the nationalist movement and as proprietor and editor of the popular nationalist newspaper, the *Bengalee.* Though a critic of the British, Banerjea remained an Anglophile, committed to the British ideals of parliamentary government and legal justice, and a proponent of reform for India through legal action and evolutionary change.

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In a scenario familiar to nationalist movements elsewhere, Banerjea was soon opposed by a younger generation of militants who rejected the English influence, turned inward toward their own traditions, considered British rule illegitimate, and sought independence through revolutionary action. As a prominent example of this viewpoint, Argov has chosen Rai, a Punjabi of the Aggarwal (banya) caste, who was educated in Urdu and Persian and was brought up in an atmosphere that was as Indian as Banerjea's was English. At the age of seventeen, Rai joined the *Arya Samaj,* a popular religious-political movement for the reform and revival of Hinduism and its defense against Christianity and Islam. Subsequently, he became a prominent leader of the extremist wing of the nationalist movement, in opposition to the moderate wing under Banerjea. In the latter part of this book Argov describes the intense struggle between these two groups for control of the Indian National Congress from 1908 to 1920.

Though in some circles it is now fashionable to conclude that, in the struggle between militants and moderates within any political movement, the militants are most likely to win, Argov argues that though the militant agitation of the extremists, rather than the constitutional methods of the moderates, was chiefly responsible for the transfer of power in 1947, it was the influence of the moderates that accounted for the establishment of parliamentary institutions in India. It is a persuasive argument.

Massachusetts Institute of Technology MYRON WEINER

THE EXTREMIST CHALLENGE: INDIA BETWEEN 1890 AND 1910. By *Amales Tripathi.* (Calcutta: Orient Longmans. 1967. Pp. xv, 246. Rs. 18.)

THIS religio-political work somewhat parallels the works on Mohan Roy by the American scholar, Charles Heimsath, who is referred to in the text. The author is a British- and American- as well as Indian-trained scholar, who is a sectional president of the Indian History Congress and history department chairman at Presidency College in Calcutta.

The transition from religion to political, ideological issues is traced. The author's style is quite sophisticated, and, in addition to his excellent prose, he shows a deep understanding of the reform and radical movements in and out of the Indian National Congress during its first generation.

Bankimchandra, Vivekananda, and Dayananda are analyzed as to their effects upon the extremists. The Western-oriented moderates get their share of evaluation in comparison. Both forms of nationalist ideology are evolved in the discussion in a sort of Aryan spiritual versus Anglo-Saxon materialist polemic. Private correspondence is analyzed in terms of the partition of Bengal as a traumatic event of the book. Both moderate and extremist participation in boycotts and in Swadeshi is traced, as well as the fringe element involved in terrorism. Economic forces, which are also shown in tables, are involved in the discussion as well as cultural and political history. The various Congress party struggles are probed during this fermentive early period up to the Gandhian era, and the author's tale ultimately focuses on Aurobindo after the political split of 1907 and the accompanying terror, with the Muslim League, Morley-Minto reforms, and other British responses to the extremist challenge traced in detail.

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Thus, the moderate reform idea is coupled with the extremist attempts to capture the Congress, despite the book's title. The communal electorate idea is explored in detail, and Minto's clever political moves are clearly revealed. The book ends with a critical analysis of the Morley-Minto Act and Regulations of 1909. Professor Tripathi here foresees the portents of future crises endangering the raj and Indian unity in the moderate disillusionment with reforms as well as in Muslim separatism.

This is an excellent work for a study of Tilak, Pal, Lajpat Rai, Aurobindo Ghose, Bankimchandra, Vivekananda, Dayananda, and others in the Hindu nationalist movement of 1890 to 1910.

Western New England College ROBERT L. BOCK

STATE POLITICS IN INDIA. By *Paul R. Brass et al.* Edited by *Myron Weiner.* (Princeton, N. J.: Princeton University Press. 1968. Pp. xii, 520. $12.50.)

PROFESSOR Weiner, the editor of this volume, is certainly one of the most thoughtful and productive scholars working on India today. Beginning in the 1950's, when little original scholarship in the social sciences had been done in the United States concerning India, Weiner produced a series of seminal political studies. We now have another important addition to this work, to which Weiner contributes a valuable sixty-page interpretive essay introducing the eight studies of individual Indian states. The conception of examining the Indian political system from the perspective of the states is also largely his. It was in the Committee on State Politics in India, organized by Weiner in the early 1960's, that many of the questions examined here were raised initially.

The one characteristic of Indian politics that comes through most clearly in all of the essays in this volume is that in the twenty-one years since independence the political process has come to be influenced more and more by socially traditional groups and less and less by Western-oriented, urbanized groups. In the Indian context, and re-enforced by universal adult suffrage, this has produced a process of rural politicization involving mass participation and the leadership of traditionally dominant elites. This introduction of indigenous social elements into the political system has meant that politics has come increasingly to reflect the characteristics of the society in which it operates. This "traditionalizing" or "indigenizing" of politics, especially at the state level and below, affects the Congress and all other political parties. It has made caste, tribe, language, and religion important political considerations and has in many respects made these social and cultural distinctions elements of political conflict. Conflict at another level derives from subregional differences and interests within states, and particularly from the competition between factional groups. It should be emphasized, as it is throughout this book, that factional politics, as it is found in most Indian states, may be based on some or all of the above factors but rarely if ever on any one alone. All of these elements of political conflict are found to operate within individual parties as well as among political parties. It is important to understand that the political system described in these terms operates within state boundaries and often within more limited subregional units to which particular social and cultural differences are confined. We have then a situation that is socially and

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politically segmented and that provides the basis around which political conflict and competition are organized. Weiner and his colleagues point out incisively that under these circumstances the critical variable in achieving and maintaining stable government is the ability of political leaders to maintain factions and coalitions of factions in positions of power. This was true within the Congress party before 1967 when that party was in a dominant position and is perhaps even more true since the 1967 elections when numerous multiparty coalitions were formed and have been re-formed in half of the Indian states. While it is suggested by the authors that the defeat of Congress in many states may lead to instability, we need to remind ourselves that the Congress both at the center and in the states is still very much alive and, nationally, the chief instrument for development and growth. As a matter of fact, Weiner raises the crucial question of how factional politics in the Indian states affects governmental performance. He concludes that, as the issues of political competition are the control of patronage, development funds, educational funds, licenses and permits, and not large questions of public policy, governmental performance in terms of economic development and modernization has not been good in the states. As a result, and despite the move to political autonomy of the Indian states vis-à-vis the center, the central government, in fact, has assumed much of the responsibility for change and development in many areas of state responsibility. Weiner contends that the key political issue in India is for the center to encourage developmental initiative and modernizing tendencies within the states.

The quality of the essays in this volume is generally high. Paul Brass's is notable for its sharp analysis of social factors motivating political change in Uttar Pradesh, the largest of the Indian states. Brass, as do most of the other contributors, also provides a substantial look at the historical antecedents on which the contemporary political situation rests. The social, analytical approach that appears here is regrettably lacking in the study of Jammu and Kashmir. This essay does not adequately treat the social, communal, and regional factors involved in the complex arena of Kashmir politics. In style it is essentially a piece of reportorial writing, and in approach it is more frankly partisan than one would like in a volume of this kind.

Each of the authors, wherever possible, follows the frame of reference laid out in Weiner's introduction, though it must be understood that the availability of data and the special competence and interest of the author determine the particular emphasis of each essay. The eight essays deal successively with Uttar Pradesh, Madhya Pradesh, Maharashtra, Jammu and Kashmir, West Bengal, Rajasthan, Andhra Pradesh, and the Punjab. Except for Andhra, South India is noticeably unrepresented. This is not to suggest an oversight or a defect in the present work, but simply to observe the existing limitations of scholarship in the field. When the companion volume dealing with the nine remaining Indian states is produced, the very significant contribution that the present study makes to the India literature will be fulfilled.

The Princeton University Press should be commended on a fine job of bookmaking and the authors on providing an extensive series of illustrations, figures, and tables to illuminate their arguments.

University of Virginia WALTER HAUSER

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WEST BENGAL AND THE FEDERALIZING PROCESS IN INDIA. By *Marcus F. Franda.* (Princeton, N. J.: Princeton University Press. 1968. Pp. ix, 257. $7.50.)

SEVERAL observers of the Indian political scene have suggested that the power of the state governments is increasing with respect to center-state relationships. In this work, Professor Franda has examined three major issues on which there were a power relationship and potential conflict between the union government of India and the government of West Bengal. On all of these issues—redefining the boundaries of the state (a constitutional power of the central government), establishing and operating a regional river development project such as the Damodar Valley Corporation, and implementing land reform (a long-established policy of the Congress party)—the view of the state government finally prevails. Indeed, the central government avoids as much as possible any direct confrontation or ultimatums seeking instead to establish some consensus and not infrequently arbitrates between factions that arise within the state.

But does he really demonstrate his thesis? Using these cases, he certainly does, although a nagging suspicion remains that the situation is not, on balance, as decisive as that presented here. A chapter summarizing many issues involving the federalizing process would have been useful.

The book is excellently researched, and the chapter on the Damodar Valley Corporation is particularly well done. This portion is also well written, whereas some of the other sections tend to be tedious and rather dry reading with little interpretation although they do provide well-documented descriptions. The book must also be applauded for avoiding the increasing, although in my judgment unnecessary, political science jargon to be found in so many books on contemporary Indian politics. Franda's work is, therefore, intelligible to both historian and political scientist.

In this continuing series of monographs on the state politics of India, Franda's book should be ranked in the general area of Nayar's study on the Punjab, Brass on Uttar Pradesh, and perhaps Bailey on Orissa. Each offers insight into state politics from a different perspective, and we shall need more of these studies in depth in order to appreciate better the diversity of contemporary Indian politics.

University of Windsor J. W. SPELLMAN

GOVERNMENT AND REVOLUTION IN VIETNAM. By *Dennis J. Duncanson.* [Issued under the auspices of the Royal Institute of International Affairs.](New York: Oxford University Press. 1968. Pp. xiv, 442. $9.50.)

IN CONTRAST to so many current bandwagon publications on Vietnam, Mr. Duncanson's study combines depth of scholarship with direct experience in the Vietnamese conflict. He presents a reasonably balanced view of French colonial rule, of the Ngo Dinh Diem regime, and of American aims and methods in Vietnam.

Diem, the central figure in the book, is seen as "the embodiment of his country's soul for good no less than for bad," as typical of educated Vietnamese in his outlook which tended to be theoretical and scholastic rather than practical and empirical. Diem's downfall resulted therefore not so much from his despotic

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rule as from his weakness as an administrator and his ignorance of the ordinary machinery of government. The misjudgment of the United States, from this viewpoint, lay in its decision that Diem's greatest needs were money and a large army when what he really required was an efficient civil service. The basic cause of the failure of American aims during this phase is seen as lack of coordination and want of direction in the application of aid and advice—precisely the strong points of the colonialism with which America wished to avoid being identified. "Defence of freedom" was very fine as a slogan, but it was quite inadequate as a practical policy.

On the larger question of why the spirit of nationalism has so far failed to provide a common focus for national endeavor in Vietnam, the author points to the historical causes of the factionalism that he terms the most constant characteristic of Vietnamese society. Centrifugal forces within that society were bred and sustained by centuries of southward-moving frontier conditions, causing a perennial conflict at the level of the village commune between the stabilizing forces of central government and the disruptive elements of local revolution. A weak central government and an absence of national cohesion thus became abiding features of Vietnamese political life.

In this light Duncanson sees Vietnamese nationalism today as "entirely negative" and "sectarian"; political and religious ideologies have served as justificatory systems for groups of leaders seeking power. And since the tradition of factionalism remains stronger than the call of nationalism, release from French colonial rule, the author maintains, would have been followed by internal division and regional partition even if there had been no Communist factor in the situation. The Communist element in Vietnam, originally introduced as a subversive movement from outside by the International, was only able to adapt itself to the local situation through the accident of the Pacific War and in this way to become a participant in the traditional Vietnamese power struggle.

Some of the conclusions drawn from the facts as presented by the author are open to debate, but on the whole this is an impressive, soundly based, and well-documented study.

University of British Columbia BRIAN HARRISON

FRAGMENTS OF EMPIRE: A HISTORY OF THE WESTERN PACIFIC HIGH COMMISSION, 1877–1914. By *Deryck Scarr*. (Honolulu: University of Hawaii Press. 1968. Pp. xviii, 367. $11.50.)

SIXTEEN years ago, at Suva, Fiji, I watched the ceremonious debarkation of a new High Commissioner of the Western Pacific. Deryck Scarr has written a close study of the evolution of this British presence in the last stage of European expansion.

Prior to the annexation of Fiji in 1874, the principal restraint exercised over British subjects in this area was the power of consuls in Fiji and Samoa, supported sporadically by the supreme courts of New South Wales and Van Diemen's Land, and reinforced by cruising British naval commanders (whose standing orders gave them powers to protect or curb Britons overseas).

The annexation of Fiji was a reaction to the growing disorders that accom-

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panied the influx of European, British, and American adventurers into the islands areas in the 1870's. In 1877 the powers of the governor at Fiji were enlarged to encompass the broader role of High Commissioner for the Western Pacific, with a limited authority over British nations in the islands of Polynesia, Micronesia, and Melanesia and an advisory role to the monarchs in Tonga and Samoa.

The High Commissioners found that their effectiveness was diminished by a lack of jurisdiction over non-British foreign nationals and that their protective attitude toward indigenous islanders sometimes clashed with the British naval policy of intervention in intertribal strife.

The High Commissioner, increasingly absorbed by problems in Fiji, was able to give only limited scrutiny to other local administrations within his directive. A gradual extension of formal British protectorates took place. In these phases the functions of the High Commissioner were chronically crippled by shortage of funds.

Periodic hopes in the Colonial Office that the Australian colonies, when federated, would accept responsibility for the Western Pacific faded after 1901. Doubts grew regarding Australian trusteeship in view of the Australian Commonwealth's position regarding nonwhite peoples. The Colonial Office belatedly accepted Sir Arthur Gordon's principle that the Western Pacific was an imperial responsibility.

The record of the protectorates proved to be no more beneficial than the earlier formulas for the island populations—except in Tonga. The advance of commercial enterprise, with its train of Asian indentured labor, had taken the center of the stage as chronic population decline in the islands continued. Land and resources, rather than the welfare of the population, became the principal objective of imperial interest.

Readers will find this a detailed, scholarly book, based upon a thorough study of sources. The subject presented the author with organizational problems—large areas, diverse and sometimes unrelated regions, and, inevitable, difficulties of chronology—that he has not entirely mastered. He does, however, offer valuable information on policy collisions between officials and the Royal Navy, British differences with France over the administration of New Hebrides, missionary rivalries, recruitment of island labor for the plantations of Queensland, and tribal and trading company politics. Gordon comes alive clearly as a benevolent aristocrat more closely identified with his island chiefs than with the imperialists of the Disraelian-Darwinian age. John Thurston and others emerge as realizable people. But the reader sometimes finds it difficult to identify particular men and subjects within the flow of the narrative.

Colgate University CHARLES S. BLACKTON

Americas

LONE STAR: A HISTORY OF TEXAS AND THE TEXANS. By *T. R. Fehrenbach*. (New York: Macmillan Company. 1968. Pp. xii, 751. $10.00.)

IN this lengthy and detailed volume, Mr. Fehrenbach has attempted the difficult task of covering the history of Texas from the prehistoric era to the heyday of

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John Connally and Lyndon Johnson. The author commendably emphasizes analysis as well as narrative and seeks to explain Texas to his fellow Texans and to Americans generally. Because of the extent of the book, Fehrenbach has not included any bibliography or footnotes, but he assures his readers that his study "was put together from hundreds of separate and sometimes disparate sources."

Since the author presents so many novel and intriguing bits of information about Texas and the United States, it is a pity that he did not have room to describe his sources. It would be interesting to learn where he discovered that Joseph Hodges Choate served on the Supreme Court or that Al Smith was an "Eastern conservative" in 1928. Equally startling is the revelation that Dan Moody served three terms as governor of Texas or that Earle B. Mayfield succeeded Pat M. Neff in the statehouse. These careless errors, and the others that crowd the book, indicate Fehrenbach's imperfect understanding of both the sources and interpretations of Texas history. Dependent as he is on inadequate or outdated secondary works, the author adds little that is fresh or that provides insight into the problems he treats. He has not explored primary sources and in some instances merely rewrites a single volume or textbook.

Fehrenbach's extended discussion of the Reconstruction period highlights the deficiencies of his research and analysis. He ignores most of the recent work by Texas and national historians and perpetuates every hoary myth about vicious carpetbaggers, depraved Negroes, and virtuous southerners. His assertion that most Texans believed slavery was dead in 1865 ignores abundant evidence to the contrary, while his examination of Republican policy, state and national, reads like a Democratic campaign tract. Fehrenbach does not understand Abraham Lincoln and his policy, northern attitudes toward Texas and Reconstruction, or the terms and purposes of the postwar amendments to the Constitution. These pages are only a repository for every outmoded cliché about the period after the Civil War.

The author has additional problems when he confronts modern Texas. He lavishes so much attention on the Indian wars of the nineteenth century that he has only a few pages to devote to the least understood and most important period of the Texas past. Perhaps the absence of readily available secondary sources accounted for this poor organization. In any case, while Fehrenbach's confused analysis of the careers of James E. Ferguson, Edward M. House, and Lyndon Johnson may amuse historians, it will not enlighten them. He handles the Negro problem in Texas like an apologist rather than a scholar, and his treatment of minorities throughout the book does him no credit.

The book's excessive length results in large part from his propensity for the gratuitous digression. Intended, perhaps, to reveal the breadth of the author's mind, it only exposes further his naïveté and limited reading. Kinder editors would have advised Fehrenbach not to rely on Theodore Roosevelt as a source for the western movement or to trim ignorant observations on constitutional history. All in all, this book is a failure. It does nothing to meet the need for a reliable, one-volume history of Texas and serves only to underscore the dangers that await the unwary dilettante who attempts the serious business of writing the history of an important American state.

University of Texas, Austin LEWIS L. GOULD

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EVERYMAN'S EDEN: A HISTORY OF CALIFORNIA. By *Ralph J. Roske.* (New York: Macmillan Company. 1968. Pp. xii, 624. $12.50.)

THE publishers of this useful survey do no one a service by describing it as the "first modern, full-length history of California." Although it adequately reviews the topics that have become standard in histories of the state, it does not surpass the best of the several textbook histories that are still in print in scholarship, style, or even significantly in length. The earlier chapters seem most traditional, telling more of Spanish and Mexican governors and their troubles than we have had since H. H. Bancroft and C. E. Chapman. About twice as much space goes to events of 1846–1900 as to those since 1918, but the last chapters go beyond the standard accounts to brief us on recent contributions to politics and culture. Roske's strength is in factual, and sometimes encyclopedic narrative rather than in interpretation.

Small errors of fact and form are excessive and suggest haste in writing and editing. For $12.50, 40 per cent more than the price of the leading textbook history of California, a publisher ought to provide better proofreading, and, in this field, by someone familiar with Spanish spelling as well as with such details as the reasons for not saying "Chief Justice of the United States Supreme Court" and "Reverend Scott." Reproduction of photographs is inexcusably muddy. There are no footnotes. In the brief "Note on Sources and Suggestions for Further Reading," the comments that go beyond books are so slight as not to be worth including. Not many would agree that T. H. Hittell's history of California is "the standard reference source and the most comprehensive history of the state."

University of Oregon EARL POMEROY

SPANISH GOVERNMENT IN NEW MEXICO. By *Marc Simmons.* ([Albuquerque:] University of New Mexico Press. 1968. Pp. xv, 238. $6.95.)

Too long have historians accepted Bancroft's claim that "from 1700 New Mexico settled down into that monotonously uneventful career of inert and non-progressive existence which sooner or later is to be noted in the history of every Hispano-American province." Such acceptance has led to vague generalizations about the agrarian character of the province, its isolation, and the attacks upon it by the Plains Indians and the French. The institutional changes resulting from the Bourbon reforms during the latter years of the eighteenth century have also been largely ignored. Scholars have tended to claim that the great distance from Mexico City to Sante Fe cut New Mexico off from "the main current of events transpiring in New Spain."

This study, based as it is upon hitherto neglected Spanish documents, proves conclusively that the distant province was directly influenced by changes introduced into its colonies by Spain in an effort to protect them from foreign design. Part I is concerned with the origins of reforms in the interior provinces of New Spain and especially emphasizes their direct effect upon New Mexico. Part II outlines the operation of the office of governor, the financial administration, and the details of the military organization. Part III evaluates the agencies of local government. There is a minimum of interpretation in the presentation; the facts alone tell the story.

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There has also been too much of a tendency to study the colonial history of the American Southwest as something apart from, when in fact it was but a small part of, Spain's far-flung empire. If anything, the threat posed by the Indians and the French and the extreme poverty of the province made the political officers in Santa Fe more dependent upon their superiors in Mexico City than might otherwise have been the case. The author has placed the government of New Mexico within the larger context of colonial Latin America where it belongs.

This book is essential for anyone interested in the colonial history of New Mexico, but it is also valuable to the student of Spain's political administration as a case study of one province. For the expansion and elaboration of the administrative system of New Mexico as a result of the reforms of Galvez and others were typical of much of New Spain and of all of the Spanish colonies. Spanish government in New Mexico during the past fifty years of colonial rule is but one example of how the Bourbon sovereigns sought in vain to tighten their political control in the New World before it was too late.

California State College, Fullerton WARREN A. BECK

AMERICAN BUILDING: MATERIALS AND TECHNIQUES FROM THE FIRST COLONIAL SETTLEMENTS TO THE PRESENT. By *Carl W. Condit.* [The Chicago History of American Civilization.] (Chicago: University of Chicago Press. 1968. Pp. xiv, 329. $10.00.)

American Building is the latest in a series of books that has given Professor Condit pre-eminence in the history of American building technology. The present volume is essentially a digest of his two-volume *American Building Art: The Nineteenth Century* (1960) and *The Twentieth Century* (1961), except that he has given somewhat more attention to colonial development and added material on nineteenth-century masonry building (especially that of the Greek Revival), which figured less conspicuously in the previous work. The terseness forced by the reduced coverage gives this book an advantage over its predecessors as an introduction to the subject. Hence, those unfamiliar with building technology may do well to start with this version before turning to the fuller treatment.

For every major aspect of building technology Condit deftly pinpoints the "firsts," the "biggests," the "innovative," the "normative," the "extraordinary, but idiosyncratic," the "masterpieces," based on far-ranging research. His discussions of technology are concise and clear, although the nonspecialist will wish that the text included explanatory diagrams of structural and engineering principles. He alludes to European contributions to American technology and mentions some of the differences between European and American developments. Europeans emphasize theory; Americans, application. Europeans concentrate on saving costly materials; Americans, on minimizing costly labor. He does make the interesting observation that Americans made exceptional contributions to the nineteenth-century development of the truss, but he barely questions what might account for this fact. Since "building" for Condit is the structural side of architecture, he minimizes two aspects of his subject that, had he considered

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them, might more fully have illuminated American achievement as compared to European. The first of these is building processes and methods, which he discusses only as they impinge on problems of structure. The second is environmental controls and mechanical equipment, which he does not discuss at all. In fairness, however, a comprehensive comparison of American and European building developments must await European surveys with the same breadth of coverage as this one for America.

Nor is it a criticism of Condit's book to add that the historian of American culture should be aware of another chapter in American building, unearthed by George Collins and published after this volume appeared. Collins investigated the fascinating and important, if rather exceptional, role of Rafael Guastavino and his son in introducing Catalan vault building techniques into many of the most familiar monumental buildings erected in the United States from the 1890's through the 1930's ("The Transfer of Thin Masonry Vaulting from Spain to America," *Journal of the Society of Architectural Historians,* XXVII [Oct. 1968], 176–201).

Finally, the historian of American culture will be grateful for the chronology of significant events that Condit includes as an appendix to his helpful text.

Brown University WILLIAM H. JORDY

THE NEW ENGLAND MEETING HOUSES OF THE SEVENTEENTH CENTURY. By *Marian Card Donnelly.* (Middletown, Conn.: Wesleyan University Press. 1968. Pp. x, 165. $7.50.)

THE GOTHIC REVIVAL & AMERICAN CHURCH ARCHITECTURE: AN EPISODE IN TASTE, 1840–1856. By *Phoebe B. Stanton.* [The Johns Hopkins Studies in Nineteenth-Century Architecture.] (Baltimore: Johns Hopkins Press. 1968. Pp. xxiv, 350. $12.95.)

LET us make clear at the outset that both these books are excellent—models of their kind. Their kind is largely, of course, historical chronicling that is concerned with recording and collating precisely defined fields of knowledge. Carefully documented, exhaustively researched, and well written, they are fine pieces of work, do credit to their authors, and add significantly to the literature on American architectural history. A review describing them would necessarily involve little more than recapitulating tables of contents. Suffice it to say that both are most valuable reference works and research tools for any library, providing, within their set limits, answers to most questions of who built what where. No longer need you fear embarrassment should someone stop you on the street and ask, for instance, "Who was Robert Tweld?" or "What do you know about St. John's Chapel, Cornersville, Maryland?" With these books at hand, you can reply confidently and immediately—well, almost immediately; for no other books, I believe, can any such claim be made.

Both are, in short, fine examples of what at this moment in architectural history is considered sound scholarship: they take implicitly for granted, as a premise not even needing statement, that "good architecture" is a matter of aesthetic taste, with due attention to expressing the realities of material and structure—or, in other words, they implicitly assume that all past architecture

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is prologue for the technological rationalism of Mies, Gropius, and Louis Khan.

Book reviewers all too frequently stand what they read up against some pet ideology of their own, mentally putting gold stars opposite pages conforming to it and black marks opposite those that do not, and I am no exception. My particular fixation happens to be with the principle that the essential difference between architecture and mere building is neither a matter of aesthetics nor of apt expression of materials and structure, still less of "reality" in the sense of technological rationalism; it is, rather, a matter of meaning. The measure of successful architecture is in the first instance effective symbolism, intrinsic or extrinsic as dictated by circumstances. No matter how appealing to the eye or serviceable to the body, unless a building in some way stands for, or shows forth, the basic convictions underpinning the institution responsible for its erection, it remains mere artifact and shelter. Of such institutions the church has traditionally been most prominent in Western civilization, and church architecture provides the most typical and consistent example of such symbolism from earliest ages to the present; one might therefore hope if not expect books such as these to have much to say on this theme. Unless I missed some passage, however, Professor Donnelly makes only brief and almost incidental reference to it, and Professor Stanton shows no awareness of its importance whatever. To a reviewer of less determined mind, such unanimity might possibly suggest some flaw in his argument, or, at the very least, confinement of comments to what the authors did choose to write about. But this omission is, I think, vital to remark and review.

Imagine a treatise on horticulture, say, that made no reference to flowers having color or fruit taste and that considered the chief function of orchards and gardens to consist of preventing soil erosion until such time as the land could be blacktopped; that is something like the situation here. For Stanton the Gothic Revival in American church architecture between 1840 and 1856 appears to be a mere "episode in the history of taste," inexplicable and without significance in historical context; its only fundamental importance, we gather, was to prepare the way for twentieth-century technological rationalism. She repeats this in various ways throughout the book. In her case it is a peculiarly unfortunate blind spot, for symbolism was the very nerve center of the whole ecclesiological movement on both sides of the Atlantic, the pivot of all its judgments, its fundamental criterion for good and bad. Indeed, many of the quotations she employs make this absolutely clear. To ignore them—to ignore the fact that ecclesiologists were part of a broad movement trying to make Gothic forms a symbol of British nationalism integral to the whole ideological reaction against the French Revolution that shaped nineteenth-century Britain, including statements of conviction about the character of evolving British institutions of law and government and making no mention of the sociological significance of so many Episcopal churches being built in the concentrated space of 1840–1856—is to ignore what must, for historians at any rate and certainly for readers of the *American Historical Review,* be the chief point of interest about the ecclesiological movement in Britain and the United States.

Donnelly's lack of systematic concern with meetinghouses as symbolic form is less damaging, for this was a less conscious concern for Puritans than for ecclesiologists. Still, I think it could have been demonstrated that, wittingly or

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not, an architectural symbol of their beliefs was precisely what the Puritans were trying to evolve, and Donnelly indeed acknowledges as much in her summation: "The Puritan contribution to Western architecture was not so much in ornament or lack of it as in a building concept suited to the needs and purposes of the New England theocracy. . . ." I would like to see her pursue this theme in a further study, and I hope that Stanton will do the same. Certainly they are the ones to do it, for, through these books, both have established themselves as authorities in these fields.

University of Victoria ALAN GOWANS

JOHN ELIOT: "APOSTLE TO THE INDIANS." By *Ola Elizabeth Winslow.* (Boston: Houghton Mifflin Company. 1968. Pp. 225. $5.95.)

JONATHAN EDWARDS: THE NARRATIVE OF A PURITAN MIND. By *Edward H. Davidson.* (Cambridge, Mass.: Harvard University Press. 1968. Pp. xii, 161. $6.00.)

To link together two such books as Ola Winslow's *John Eliot* and Edward Davidson's *Jonathan Edwards* is not an easy task. They are similar in that their subjects are both Massachusetts ministers. They are also Puritans if you will, the first from the high point of the Puritan period, the second put forth as an ardent defender of Puritanism in the years of its decline and fall, as the logician *par excellence* who managed to combine brilliantly if not effectively enlightened science and an old theology. They are similar, too, in that in both the authors must account for some very un-Puritan preaching on the part of their subjects: Eliot preaching a rank Arminianism to his Indians, Edwards verging on a universalism during the Great Awakening. And they are similar, finally, in that both authors are somewhat unhistorical in their attachment to their subjects. For Winslow, Eliot is a democratic, humanitarian, and American hero. For Davidson, Edwards is an inevitably true prophet for his and, curiously, our age. How else is one to read such lines as: "That few in his own day heeded him [Edwards] is only an admission that any age ignores its own best thought and lives comfortably, albeit foolishly, within its dark ambiguities and confusions"; or, "Yet there was power and there was truth in what Edwards proclaimed in 'Sinners.' The truth was, and is, that man must be reborn in order to know himself"?

Beyond these similarities, however, there is little to link the volumes. *John Eliot* is Winslow's well-known brand of narrative biography at its best. It is commendable in that it hints at basic problems of cross-cultural contact (Eliot's pronouncements and the Indians' understanding); it is to be criticized for numerous errors (a ludicrous account of John Cotton's role in the Antinomian controversy, for example) and for its simplistic hero worship. Davidson's *Edwards,* published in 1966 as part of the "Riverside Studies in Literature," was largely ignored by historians and has now been republished by Harvard University Press. It is a narrative only in subtitle and in the consideration of Edwards' works in chronological order of composition. More appropriately one labels it an exegesis of those writings. As such it is commendable for its relatively simple style—I stress the qualification, for any consideration of Edwards is necessarily complex—but it

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adds little to the total of Edwardian interpretation. Moreover, the very fact of its being an exegesis constitutes a basic flaw. The work is so intensely focused on Edwards' own writings that the continuities between Edwards and his past tend to be forgotten. Thus the notion of the invisible church, for example, appears rather absurdly as peculiarly Edwardian when first introduced by the author, an appearance of absurdity only partially cancelled by a subsequent quotation from William Ames's 1636 *Marrow of Sacred Divinity*. In the end, one can only conjecture that the book's relative simplicity, the fact that it is a good introduction to Edwards' writings, and the very proper feeling that it deserves somewhat more attention from historians than it received on its original publication led to its republication.

University of New Hampshire DARRETT B. RUTMAN

A NEW VOYAGE TO CAROLINA. By *John Lawson*. Edited with an introduction and notes by *Hugh Talmage Lefler*. (Chapel Hill: University of North Carolina Press. 1967. Pp. liv, 305. $10.00.)

JOHN Lawson moves on the pages of history for little more than a decade. There is no certain evidence of him prior to his coming to America, and even his family background is a matter of controversy. But if the beginning of his career is shrouded in obscurity, its hideous termination in 1711 at the stake of the Tuscarora is all too well authenticated.

Shortly after his arrival in Carolina, Lawson was engaged by the proprietors to make a survey of the back country. With a small group of companions, the personnel of which changed several times, he set out when the seventeenth century had less than four days to run. Proceeding inland up the valley of the Santee, he swung in a bold arc through the Piedmont Region and regained the coast by way of the Pamlico River. His account of this journey was doubtless expected to be promotional as well as informative, and it must surely have satisfied his employers on both scores for it is a glowing picture that he paints of this yet scarcely disturbed country. He was himself a keen and interested observer, with that wide range of interest and information that was more possible in his day than in ours. He describes the lay of the land, its geology, the flora and fauna, and the aborigines and points out as well the possibilities for the development of many of the sites that he observed. Like the pioneers of over a century earlier, he was impressed by the teeming bird life of the region and bears witness to the abundance of at least two species, the Carolina parakeet and the passenger pigeon, which are now extinct.

The information, which appears somewhat incidentally in the diary, is systematized and extended in other sections of this edition, and additional topics are similarly treated. Lawson's "Description" includes instructions for negotiating Carolina's tortuous harbor entrances that would be worthy of a modern *Coast Pilot,* and his "Present State" delineates the progress and activities of the European inhabitants. The result is a composite work, and its various editors have thus had some latitude in selecting its title. The one employed here is taken from the first edition—the only one published during Lawson's lifetime.

The present edition is a handsome volume. The map of the first edition is

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reproduced, and the illustrations of animal life, which were earlier crowded into a single plate, are here distributed artistically throughout the volume. Other illustrations have been added, and one is gratified by the somewhat anachronistic inclusion of the superb pictures of Indians that John White sketched over a century earlier. The footnotes are most helpful though one might quarrel with them here and there. The failure to identify species of flora and fauna, admittedly a hazardous undertaking after more than two centuries and a half of drastic modification by European occupation, is also regrettable since this approach might have edified the biologically interested but untutored reader. Yet, this is an excellent edition of a classic that in both form and content has well deserved its enduring popularity.

University of Pennsylvania LEONIDAS DODSON

DUTCH CALVINISTIC PIETISM IN THE MIDDLE COLONIES: A STUDY IN THE LIFE AND THEOLOGY OF THEODORUS JACOBUS FRELINGHUYSEN. By *James Tanis*. (The Hague: Martinus Nijhoff. 1967. Pp. ix, 203. 23.50 gls.)

THIS book contributes importantly to the growing body of literature on religion in colonial America. It recounts the life of a major figure in colonial church history and dissects in fine detail his theological heritage and contributions. For that reason it is not an easy book to read; it is more a work of theology than of biography, and its style is heavy, often pedantic. The rewards, however, are considerable. From this slim volume one can learn much about the origin of pietistic doctrines in Europe and their spread through the Middle Colonies of British America. Along the way the reader becomes acquainted with a figure whose name is familiar but whose beliefs and contributions are usually treated tangentially if at all.

James Tanis, librarian of Yale, has divided his study into two parts of almost equal length. The first takes Theodorus Jacobus Frelinghuysen (1692–1747) from his youth in the Ruhr Valley through student days at the University of Lingen, a pastorate in East Friesland, and, finally, in 1720, to America where he spent the rest of his life meeting the spiritual needs of Reformed Protestants in the Raritan Valley of New Jersey. Because he had more talent than tact, Frelinghuysen led an often stormy life, but his impact, Tanis argues, was profound. His influence extended beyond the Dutch Reformed parishes to the Presbyterian Tennent family and, indirectly, to the Anglican George Whitefield. Part Two examines in detail Frelinghuysen's "experimental divinity," with compact analyses of the pastor's beliefs on topics such as the covenant of grace, infant baptism, heaven, hell, and church discipline. In this section of the book, as in the other, Tanis bases his findings on a thorough use of both American and European writings, principally those of Frelinghuysen and his contemporaries.

It is both the strength and the weakness of this volume that it is rather narrowly concerned with Frelinghuysen himself. By keeping the focus on his subject, Tanis has avoided the labyrinths of larger historical questions. At the same time, by concentrating so heavily on the life and thought of a single

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figure, Tanis' book may leave some readers, as it left me, with a sense of having encountered Frelinghuysen *in vacuo*.

Columbia University ALDEN T. VAUGHAN

ANTEBELLUM NATCHEZ. By *D. Clayton James.* (Baton Rouge: Louisiana State University Press. 1968. Pp. xiv, 344. $10.00.)

HERE is an account of the history of Natchez from its founding in 1714 through the secession movement of 1861. It provides close insight into the life of the town's people, debunks many myths about Natchez "under-the-Hill," and evaluates the role played by Natchez in the history of the lower Mississippi Valley, of the Old Southwest, and of Mississippi. It is based on careful research, on a wide variety of sources, and on an objective attempt to tell the truth as the author sees it. For his trouble, he will receive the blessing of the history profession, but, unless I miss my guess, the eternal damnation of the United Daughters of the Confederacy, the Natchez Pilgrimage Association, and other assorted myth-makers.

In three broad chapters James examines the early history of Natchez under French, British, Spanish, and American rule, explaining how first tobacco and then cotton became the foundation of the town's fabulous wealth. In the next eight chapters he gives us a critical look at various aspects of Natchez in its ante bellum years: the town's government and ruling class; the nabobery or aristocracy; the voting patterns; the other people in Natchez; the failure of the town to grow; the town's intellectual, cultural, and social history; and, finally, the town's exciting years and "excitable citizens."

James makes a valuable contribution in his analysis of voting behavior in Natchez. We have always assumed that Natchez was a Federalist-Whig stronghold. James has found only part of the precinct votes for ante bellum Adams County, but those he has found demonstrate that Natchez was a humble island of Republican-Democratic voters surrounded by a sea of Adams County Federalist-Whigs.

The town government was run politically by Republican-Democratic middle-class merchants and professional men on the make. But there was a privileged class, a nabobery, in Natchez that possessed great economic wealth and power. In spite of considerable vagueness in regard to the actual names of most of the forty-odd families forming this class and to what is really meant by the term aristocracy, James's analysis of the Natchez nabobery seems to be both fair and sound. It goes about like this: The Natchez nabobs, unlike the country gentry of Adams County, formed a "more varied" economic class. They included "commercial and professional aristocrats" as well as "landed gentry." But the commercial and professional aristocrats of Natchez also owned plantations and, regardless of vocations, grew cotton and sugar. The forty-odd families of great wealth, proud of their status, looked down their long aristocratic noses at the middle and lower classes, withdrew into their own privileged world, and after a time even pretended to be an aristocracy of heredity with kinship and blood becoming highly significant. They spoke only to each other, to their "niggers," and to God and threw up social barriers that were almost impossible to breach. The

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nabobs were selfish money-makers with little or no sense of *noblesse oblige,* of civic pride, or of charity. They resented the municipal rule of the "nobodies," protested high taxes, but divorced themselves from political recourse. They could not be bothered with politics, so busy were they whipping slaves and growing cotton, managing their numerous plantations and investments, and entertaining each other.

The nabobs would not spend a cent of their money for building Natchez. And the nobodies, trying to become somebodies by investing all their money in land and slaves, would not spend their money on building Natchez either. Consequently, Natchez did not live up to its early promise of becoming a great Mississippi River trading center. Bypassed by the railroads that brought commercial and population growth to Memphis, Vicksburg, Baton Rouge, and New Orleans, Natchez became isolated and was consigned to the obscurity it so richly deserved.

University of Arkansas WALTER L. BROWN

NEGRO TROOPS OF ANTEBELLUM LOUISIANA: A HISTORY OF THE BATTALION OF FREE MEN OF COLOR. By *Roland C. McConnell.* [Louisiana State University Studies, Social Science Series, Number 13.] (Baton Rouge: Louisiana State University Press. 1968. Pp. xi, 143. $5.00.)

INTEGRATION OF THE NEGRO IN THE U. S. ARMED FORCES. By *Richard J. Stillman, II.* [Praeger Special Studies in U. S. Economic and Social Development.] (New York: Frederick A. Praeger. 1968. Pp. xii, 167. $10.00.)

THE role of the black man in the military is among the many areas being examined in the rapidly mushrooming "field" of American Negro history. To the extent that one is an activist in the cause for equal rights, the temper of the works ranges from a low-key scholarly approach as exemplified by McConnell to the more exhortative appeal made by Stillman.

Relying heavily on archival material, McConnell offers a favorable account of the deeds and valuable services of "a unique organization" in Louisiana—the Battalion of Free Men of Color—from its beginnings in 1729 through the War of 1812. It was indeed unique in such a slaveholding region, with constant rumor and fear of slave insurrection, that whites would entrust blacks with weapons. But the critical dangers of 1729 and the shortage of trained white soldiery compelled the French to employ Negroes against the fierce Natchez and Chickasaw Indians. Similarly, when the territory changed hands, the unit fought gallantly for the Spanish against the British in the late 1770's. As is well documented by McConnell, the various European nations that controlled Louisiana in the eighteenth century came to respect the loyalty and valor of the Negro detachment.

The action taken by the Louisiana legislature in 1812, which authorized a Negro volunteer militia, was unprecedented in American history. Though segregated, the Battalion of Free Men of Color had its own commissioned Negro line officers and received the same pay and treatment as white groups. The unit's creditable performance, especially in defense of New Orleans, won plaudits from

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General Andrew Jackson as well as federal and state pensions and military land bounties for many of its members.

Unfortunately, owing to the lack of diaries and other personal papers written by the soldiers themselves, McConnell was unable to investigate the attitudes and feelings of the black militiamen. A question that remains unanswered is how they reacted as human beings to the separate, though equal, military system. This minor criticism aside, however, the volume compares favorably with earlier military accounts by Benjamin Quarles, Dudley T. Cornish, and Ulysses Lee.

In a broader approach to the role of the Negro in the United States armed forces, Stillman surveys past progress and current problems from the political as well as the military standpoint. The author stresses the fact that, while the military has been integrated for over a decade and leads the nation in providing opportunities for the blacks, much remains to be done before true equality is a reality.

In a two-chapter survey, Stillman discusses race relations in the military since the American Revolution as a means of providing background information. This is a thinly researched, cursory review that could have been excluded without detracting from Stillman's main consideration, which is the investigation of the present Negro-military relationship. To note but one enduring discriminatory practice, Negro soldiers still suffer the indignity of off the post housing segregation, particularly in southern states.

From an academic point of view, this volume is not well written. It is inadequately researched, has no index, and, though an annotated bibliography is included, most of its sources do not relate directly to the subject. Despite the book's weaknesses, Stillman must be credited with getting his message across to the reader, perhaps more effectively than the more impeccable, though less dynamic, McConnell. Both authors have shown that the Negro was and is a credit to the nation's armed forces. That much more remains to be done is further illustrated by Stillman—amateurishly but convincingly.

Boston College ANDREW BUNI

A ROPE OF SAND: THE COLONIAL AGENTS, BRITISH POLITICS, AND THE AMERICAN REVOLUTION. By *Michael G. Kammen.* (Ithaca, N. Y.: Cornell University Press. 1968. Pp. xviii, 349. $10.00.)

COLONIAL agencies in London were a developing institution that, like feudalism, began to decay before it developed. The agents were the nearest approach to American representatives, but the question of whom they represented was never settled. They did not evolve into a cohesive group of spokesmen for the colonies, and in the crisis before Lexington they ceased to have anything to say. Professor Kammen analyzes them in their environment and shows how their eventual failure meshed with that of "an entire network of formal and informal lines of imperial communication." He deals with much the same material that Professor Jack M. Sosin covered in *Agents and Merchants: British Colonial Policy and the Origins of the American Revolution, 1763–1775* (1966). But the two authors approach the subject very differently: Sosin's approach is chronological; Kam-

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men's is topical within two periods, 1756–1766 and 1766–1775. Kammen is more concerned than Sosin with the intricacies of British politics, more cautious in generalizing, and more given to long exposition.

A Rope of Sand, despite its title, is not easy reading. The author buttresses his argument with factual details that are often hard to digest: his "profiles" of the agencies, for example, in the two periods (Chapters II and VIII) are masses of biographical data in the best post-Namier tradition of indigestibility. On occasion he argues obscurely, as when he imputes British ignorance of America to misleading reports from colonials, then adds that "the problem also arose from a subtly growing difference of opinion over the role and importance of colonies." (Was ignorance either the problem or the result of differing opinions?) But such obscurity is rare and more than outweighed by well-documented conclusions. The reader who perseveres to the end is solidly rewarded.

In the repeal of the Stamp Act the agencies reached their apogee. Thereafter, while the colonies developed greater cohesion, the agents showed less and less. One reason, according to Kammen, is that the great debate over the constitutional issue separated those agents who were colonials from those who were British; another reason is that the colonies, as they grew disillusioned with what could be done in London, emphasized communication between themselves more than with the mother country. The agents finally became the means, as the author shows, not of ameliorating the quarrel but of embittering it: ministerial anger was vented on them, and by reporting it to their constituents they hardened the will to resist. "By whom shall we hear the colonies," Burke had asked, "if not by their agents?" The answer proved to be by the minutemen.

University of Michigan WILLIAM B. WILLCOX

THE BIRTH OF A NATION: A PORTRAIT OF THE AMERICAN PEOPLE ON THE EVE OF INDEPENDENCE. By *Arthur M. Schlesinger.* With an introduction by *Arthur M. Schlesinger, Jr.* (New York: Alfred A. Knopf. 1968. Pp. viii, 258, xi. $7.95.)

WHEN he died in 1965, Arthur M. Schlesinger, Sr., had become something of a symbol of the changing interpretation, spanned by his own lifetime as a scholar, of American colonial society and the American Revolution. Schlesinger began his career as a progressive or "new" historian with the publication of two books, *The Colonial Merchants and the American Revolution* (1918) and *New Viewpoints in American History* (1922). In both he depicted colonial society as highly aristocratic, followed Carl Becker's "dual revolution" thesis that the Revolution was a social revolution for democracy as well as a war for independence, and accepted Charles A. Beard's contention that the Constitution was a conservative counterrevolution designed to undo the democratic gains of the Revolution. Yet in this book, published posthumously by the Schlesinger family, Schlesinger almost, but not quite, reverses his previous interpretation and follows one that has been emerging since World War II.

Twelve of the fourteen chapters—all except Chapters IX and XIV—can be recommended without qualification. In line with the hallmark that has characterized his work for so many years, Schlesinger has written a social history of

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the American people on the eve of the Revolution. Such topics as the physical and human setting, the family, country life, town life, labor, religion, intellectual life, education, the fine arts, and amusements provide the ideas for various chapters. The content is an amazing collection of information about the daily life of the people in general that could have been collected only by a scholar who had read the newspapers as perceptively as Schlesinger did. The result is reading that is always informative, delightful, and often amusing as the author engages in a little windowpeeping back into the eighteenth century.

More important, however, is the tremendous change in the milieu, the climate of opinion, of eighteenth-century America as found in the 1918 and 1922 books as compared with this one of 1965. Then American society was aristocratic, class-ridden, and filled with unfranchised lower classes and privileged upper classes, which meant that conflict rather than consensus was the order of the day. By 1965, however, early American society had become something of a poor man's paradise in Schlesinger's eyes. There was almost unlimited economic opportunity for poor people, except slaves, to get ahead whether they lived in the country or in one of the few towns. There were distinctions in economic status, but the rank and file held no resentment because they expected to get ahead as others had done. Progress was not what the unknown future held, but the heartening improvement over the past that was visible to all, for men already enjoyed "by Old World standards a heaven on earth." The only class struggle at the time was the struggle to climb out of a lower and into a higher class. Widespread political rights, religious toleration, and the best educational system anywhere for the common man all helped to make colonial America an open and mobile society, with consensus rather than conflict the outstanding feature.

It is this portrayal throughout every chapter of a democratic, middle-class, open society that raises questions about Chapter IX. The title is "The Aristocrats," yet even within the chapter itself there is a constant battle between Schlesinger's efforts to create a colonial "aristocracy" and his numerous statements and copious evidence that there was no aristocracy. The term aristocracy has the connotation of a privileged class whose power was beyond the reach of the common people, but in eighteenth-century America no such group existed. There was a group of appointed officials, such as Thomas Hutchinson, who held office, not through aristocratic privilege, but through appointment by the British government to carry out British policies. But Hutchinson lost a popular election in 1749 in Boston where, as he said, he could get only two hundred votes out of seven hundred, and where government was by numbers, not by weight. In Virginia, Landon Carter was "kicked" out of the House of Burgesses because he did not cultivate the popular vote, and his son Robert Wormley was also defeated even though he had "kissed the arses of the people" to win popular support. Schlesinger's evidence shows about the same thing, which certainly is not proof of an "aristocracy."

Somewhat the same criticism can be leveled at Chapter XIV. Here Schlesinger almost, but not quite, has a democratic America revolting from an aristocratic Great Britain in order to preserve democracy rather than to gain it. He is correct in stressing consensus among Americans against the British, although I believe that the consensus was even greater than he indicates. But, conversely, I

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doubt that the attachment of Americans to Great Britain was as great as he pictures. One should not be surprised that Americans defended their position in terms of "British liberties," for they would have gotten nowhere by insisting on "American liberties." As Schlesinger's evidence shows, however, and as many contemporaries said, Americans already had far greater liberties than the British, and when they demanded British liberties, they really meant what *they* had or what the British people *ought* to have. The evidence all indicated a liberal revolution to preserve a democratic society, yet Schlesinger concluded that democracy and nationalism followed rather than accompanied the Revolution.

If he could not quite come all the way to the newer interpretation, this book indicates that Schlesinger had certainly come a long way in that direction from 1918 to 1965.

Michigan State University ROBERT E. BROWN

A TIME TO REND: AN ESSAY ON THE DECISION FOR AMERICAN INDEPENDENCE. By *John M. Head.* (Madison: State Historical Society of Wisconsin. 1968. Pp. xvi, 208. $5.95.)

BASED upon studies of the careers of all delegates who served in the First and Second Continental Congresses through July 2, 1776, this volume seeks to re-evaluate the conflict over independence through aggregate analyses of the backgrounds and experiences of the delegates and surveys in depth of the careers of four representatives—John Dickinson, John Adams, Thomas Jefferson, and John Rutledge—who, in the author's judgment, illustrate the full "range of views on empire or independence."

In several respects, the result is highly exasperating. To begin with, the book is difficult to classify and, consequently, to assess. It is the outgrowth of a dissertation, but it bears little resemblance to the many monographs that conventionally derive from dissertations, except possibly in its prose style. It contains an appalling number of simple factual and interpretive errors; it appears to have been built upon an extremely narrow base of reading and research and lacks the meticulousness, solidity, and thoroughness usually associated with the better monographic studies. The author himself describes the volume as an essay, but that designation is only partially accurate. An essay is usually a short, personal, not necessarily complete, and tightly constructed argument. Although this volume is personal and incomplete, it is relatively long, loosely argued, and frequently discursive. An additional difficulty arises from the fact that much of the analysis seems to proceed from the debatable assumption that independence was the central issue before Congress and the colonies from the passage of the coercive acts in early 1774 until the decision for independence and that members of Congress can be adequately categorized according to their position on that single issue.

What is most perplexing, however, is that these difficulties in form and substance may prevent readers from giving the author's central thesis about the social bases of the debate over independence the attention it deserves. That thesis can be briefly summarized by the complementary formulas: internal unity plus a sagging economy equaled a tendency to favor independence, while internal

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disunity plus prosperity equaled a predisposition to oppose it. The author adduces just enough evidence in support of this thesis to suggest that it is worth very serious consideration. It will need to be much refined and extended before we will know just how much explanatory power it may have, but it strikes me as potentially the most fruitful hypothesis yet advanced to account for regional variations in response to the decision for independence.

Johns Hopkins University JACK P. GREENE

SOLDIERS & CIVILIANS: THE MARTIAL SPIRIT IN AMERICA, 1775–1865. By *Marcus Cunliffe*. (Boston: Little, Brown and Company. 1968. Pp. 499. $12.50.)

SOMETIMES it is important to remember that we Americans have been involved in *every* world war since the 1600's. In this present volume, Marcus Cunliffe, professor of American studies at the University of Sussex, England, is concerned with the development of the American military ethos in the vital period from Lexington to Appomattox.

Soldiers & Civilians will appeal chiefly to the nonspecialist, although there are incisive analyses and original observations that will interest the expert. There is little new here; most of it has been ably covered before by such military historians as Emory Upton, T. Harry Williams, Russell Weigley, and C. J. Bernardo and E. H. Bacon. The volume is fairly well documented (the notes, unfortunately, are in the back of the book), though it is almost totally lacking in the use of the voluminous manuscript sources so readily available. While the illustrations are well selected, there is regrettably no bibliography. Although rambling and diffuse on occasion, and with too many petty and unnecessary details, the book is on the whole well written. The author's assessments are usually cautious and judicious, albeit occasionally hedging.

Correctly asserting that the United States was "martial but unmilitary" in the period 1775–1865, Cunliffe goes on to discuss capably the social life of the pre-Civil War officer and ranker, the pitiful condition of the militia, the rise of the volunteer system, the peace movement before 1861, the achievements and machinations of James K. Polk, the cult of violence in this period, the development of paramilitary organizations, the equal influence on West Point of northerners and southerners, and the impact of politics on the army and vice versa. And the author is certainly correct in emphasizing that the nation has never, in its history, had anything to fear from the Regular (standing) Army.

While there is much to praise, military historians will inevitably disagree with some of Cunliffe's contentions. I, for example, must take sharp issue with the author's frequent denunciations of Winfield Scott without due appreciation for Scott's numerous and invaluable services to the republic. Nor can I go along with Cunliffe's glowing appraisal of Alden Partridge or his criticisms of Sylvanus Thayer. Also, unfortunately, the author practically ignores the parallel story of naval affairs.

This work should, however, appeal to interested lay readers or undergraduate students seeking a valuable introduction to the development of the American

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military tradition in the days before the Civil War gave greater permanency to that tradition.

Pennsylvania State University WARREN W. HASSLER, JR.

ISAAC COLLINS: A QUAKER PRINTER IN 18TH CENTURY AMERICA. By *Richard F. Hixson.* (New Brunswick, N. J.: Rutgers University Press. 1968. Pp. xi, 241. $8.00.)

JOHN Wesley Jarvis' portrait of Isaac Collins (1746–1817), painted near the end of his forty-six years of printing and publishing, has "hardly a hint of a smile." The written portrait is humorless too. Add gray writing to a gray character with no enlivening family papers available, fail to remove the scholarly scaffolding or to make the background evoke the meaning, and you have a dull book. This is a pity, for the author is an enthusiast for the history of journalism, skillful at filling gaps by deduction, and full of the facts of Collins' exciting generation.

The documented data prove the printer, omitted from the *DAB,* to have been a significant Middle States figure during the revolutionary era. Collins served the patriot cause as publisher of the *New Jersey Gazette,* the first substantial weekly in the state, from 1777 to 1786. The sixty pages on Collins' paper comprise a history of New Jersey journalism in the eighteenth century, lacking only a statistical analysis of the *Gazette*'s space. As the official state printer from 1770 to 1789, Collins published some 134 government documents. In Philadelphia (1770), Burlington, New Jersey (1770–1778), Trenton (1778–1795), and New York (1796–1807), he also published 145 books and pamphlets, two-thirds of which were religious. His quarto Bible, King James Version, appeared a month or so after Isaiah Thomas' Bible early in 1792.

Of Quaker origin, Collins was a compromising, worldly type of Friend, who was disowned for a decade (1777–1787) because he actively supported the Revolution. For his last twenty years he was both Quaker printer and committeeman. We are not told that he was an elder or minister, or that he ever aspired to leave his provincial capitals and see the world, but he did work steadily and stolidly for education, temperance, and emancipation. He married a "ranking," that is, well-to-do, Quakeress and had fourteen children, one of whom married the Quaker minister Stephen Grellet. He left an estate of $36,000.00 and a flourishing printing business with a strong medical interest.

The material in the back of this attractively designed volume contains a valuable list of Collins imprints, notes showing acquaintance with the pertinent literature, and an analytical index.

University of Vermont T. D. SEYMOUR BASSETT

ANDREW LAW, AMERICAN PSALMODIST. By *Richard A. Crawford.* [Pi Kappa Lambda Studies in American Music.] (Evanston, Ill.: Northwestern University Press. 1968. Pp. xix, 424. $12.00.)

IN 1966, Pi Kappa Lambda National Music Honor Society published *The Anthem in New England,* by Ralph T. Daniel, as its first book in a projected series of studies of American music. The present handsomely bound volume, which is copi-

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ously annotated and has a comprehensive bibliography, is the second publication of the series. Law (1749–1821) is a significant figure in the history of early American music. Through his teaching, as well as his publications, which Professor Crawford analyzes and collates in six appendixes, Law led the postrevolutionary movement to reform American psalmody in the direction of European practice. Not only did Law popularize a more sophisticated harmonization, but also, following European models, he led in the introduction of solo hymnody by which the melody, heretofore carried in the tenor part, was transferred to the more florid and ornamented soprano. Law established singing schools throughout the length and breadth of the land; he invented a new "shape notation" to simplify the reading of music; he was the first American musician to seek protection under both state and federal copyright legislation; and "he was his generation's most prolific compiler of tunebooks, publishing, in all, more than thirty separate items."

Drawing principally upon the recently discovered Law Papers now housed in the William L. Clements Library, Crawford portrays a singularly unattractive figure. A cramped and vindictive Connecticut divine, whose activities were nationwide, Law displays the meanest aspects of Puritan provincialism. Much of the book recounts the meaningless and sordid details of Law's jealous and self-righteous antagonisms toward fellow musicians, including his own pupils. Law's famous singing schools, in which he promulgated a style so refined that the music emerged as a kind of seraphic whisper, appear at times to be little else than publicity schemes to further the sale of his many books. Unlike his friend Noah Webster, Law lacked "either the ability or the inclination to extend the limits of his understanding." Lacking the virtue of Christian charity, his mind was bound within the narrow confines of Calvinist Christianity.

Law's career, we are told, "stands as an illustration of some of the forces which shaped American cultural life in the eighteenth and nineteenth centuries." Had Crawford made a greater effort to place Law in that context, his book would have greater value as a contribution to cultural history. As it is, we see Law excoriating American psalmody at the very time when William Billings was writing an exuberant chapter in the history of American music. Law, reflecting the cultural conservatism of the postrevolutionary establishment, turned away in horror from the native product to a European tradition not of Mozart and Beethoven, about whom he knew nothing, but of a "moribund European hymnody." It is fitting that the height of Law's career was his leading the singing at the Society of the Cincinnati-sponsored Washington memorial service in Philadelphia. His aesthetic principles, as reactionary as was the moribund Federalism of the Cincinnati, mirrored an elitism that would dominate American intellectual life well into the nineteenth century.

San Jose State College THOMAS WENDEL

CHARLES MORGAN AND THE DEVELOPMENT OF SOUTHERN TRANSPORTATION. By *James P. Baughman*. (Nashville, Tenn.: Vanderbilt University Press. 1968. Pp. xxxi, 302. $10.00.)

CHARLES Morgan (1795–1878) was a New York shipowner and operator who specialized in the trades of the Gulf of Mexico and who pioneered in the use of

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steamships. Through partners he operated a shop that built ship engines in New York. Trade between New Orleans and Texas ports occupied much of his time and attention during a very long business career, but he also engaged in other Gulf trades, including those between New York and Gulf ports of the United States and of the Latin countries. Morgan freely delegated authority and responsibility to sons, sons-in-law, and other partners, but he himself was the heart and soul of his enterprises. Through the vagaries of economic development, political change, and war, Morgan adapted his strategies to changing hazards and opportunities and made a large fortune. He served both sides during the Civil War, and he battled with Commodore Vanderbilt for control of the Nicaraguan trade and of the Nicaraguan route to California. He could play a rough game with competitors, but his chief strength lay in able management, shrewd judgment, and a remarkable ability to adapt his strategy to changing circumstances. Instead of blindly fighting railroads as competitors, Morgan entered into agreements with them and in time became himself an important promoter and builder of railroads in Louisiana and Texas. The operations of his railroads and steamships were carefully coordinated. In 1883, several years after Morgan's death, his railroad and steamship lines were acquired by Collis P. Huntington and became a part of the Southern Pacific Railroad system, which continued to use the term "Morgan Line" to designate its steamships as late as 1939.

Professor Baughman has produced a readable story that illuminates many subjects. He has been diligent in examining widely scattered sources, and extensive tables that are included in an appendix add details not suited to presentation in the text. A serious deficiency, however, is the fact that the author lacks understanding of the changing commercial forces at New Orleans and of the dynamic evolution of the interior trade of the United States (except for Texas). Many blank areas have had to be covered lightly, and a thick gloss of economic jargon purports to show Morgan as the embodiment of the prevailing concepts of entrepreneurial historians. I find the author's rationale unconvincing. The book's merit lies on the factual side; it enlightens areas previously obscure, and it eases the task of many scholars who must follow.

University of Alabama JAMES F. DOSTER

MELODRAMA UNVEILED: AMERICAN THEATER AND CULTURE, 1800–1850. By *David Grimsted*. (Chicago: University of Chicago Press. 1968. Pp. xii, 285. $8.95.)

THE preface to this remarkable book clearly sets forth the author's purpose: "to use the theater of the early nineteenth century to explain the popularity of that most banal of dramatic forms, the melodrama, and to glimpse that most devious of historical objects, the popular mind of an age." His success in achieving his design renders his work eminently worthwhile, but he has gone beyond even these ambitious bounds. *Melodrama Unveiled* (a somewhat misleading title) covers practically every aspect of the American theater of the period, from acting and stagecraft to audience and critic; from trends in the nature of programming to the quest for a national drama. The book manifests impeccable scholarship and a felicitous prose style throughout.

The focus, however, remains on the plays, and it is here that Professor

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Grimsted demonstrates a fine originality of conception and method. After assessing the "Good Old Plays"—plays from earlier periods that maintained their popularity—he turns to the melodrama and the central thesis of his book. Melodrama, he argues, was the contemporary popular dramatic form of a period in which theater was the most democratic of all entertainment media. As such, its enormous vogue reveals much of the ethos of the times. The structure and conventions of the melodrama, its content and its language, were in perfect tune with "the intellectual preconceptions of the period . . ." and formed an "amalgam of its social virtues." Melodrama spoke for an age and to an age when "nature's nobleman," the common man, appears as hero, while chaste and virtuous woman emerges as high priestess of a "religion of domesticity" litanized in the dialogue.

The plays themselves were "sermons in dialogue," depicting what life ought to be if not what it was, expressing a reality "transformed to accord with what was simultaneously man's daydreams" and what he saw as "the world's transcendent truth." Melodrama provided an emotional coefficient of the intuitive common-sense philosophy that dominated American thought, while offering the comfort of certain providential guidance toward virtue rewarded and vice punished. The argument, amply documented, is at once fascinating and persuasive. One might quibble with Grimsted's nationalizing of what would seem to be an essentially international phenomenon. This cavil notwithstanding, *Melodrama Unveiled* is without question the finest single volume available on the theater and drama of the ante bellum United States, and, more than that, it is "Culture History" of the first order.

University of Southwestern Louisiana JAMES H. DORMON, JR.

THE FRONTIER AGAINST SLAVERY: WESTERN ANTI-NEGRO PREJUDICE AND THE SLAVERY EXTENSION CONTROVERSY. By *Eugene H. Berwanger.* (Urbana: University of Illinois Press. 1967. Pp. viii, 176. $5.95.)

THE thesis of this short monograph is quite clear: not only humanitarianism but also strong anti-Negro prejudice among western settlers and their fear of black hordes descending upon them contributed to the growth of antislavery extension sentiment as a means of keeping Negroes out of the western states. Most of the book is a state-by-state examination of the depths of that prejudice and the consequent attempts to deny Negroes social and political rights or to exclude them by the law entirely from the Old Northwest, Pacific Coast, and High Plains states. Each state's experience seems to be a copy of the one preceding, and the materials and techniques of repression become devastatingly familiar through repetition.

Berwanger's description of the phenomenon of anti-Negro prejudice usefully supplements the recent work of Leon Litwack, Jacque Voegeli, and Winthrop Jordan, among others. But to go beyond this and to argue that such antisocial attitudes led to significant support for antislavery extension policies is not necessarily demonstrated either by developing the amount of Republican support in these areas and correlating the two or by arguing the point from the mass of often contradictory manuscript evidence. There is really little beyond the assertion to suggest that anti-Negro groups were attracted to the Republican party

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because of its attempts to limit the spread of slavery, and there is much to sustain an opposite thesis. Many westerners, including some cited here, were both anti-Negro and anti-Republican. As Berwanger correctly notes, furthermore, the Republicans were constantly in trouble in their earlier years because of their public identification as the party of potential Negro equality. Such attitudes were not overcome by the many conservative statements of party leaders. Finally, much Republican electoral support arose from reasons not directly related to the slavery crisis: economic problems, ethnic antagonisms, and so forth. Thus, while it is undoubtedly true that many Republicans were anti-Negro, it is oversimplified to suggest that they were Republicans because of that fact and a consequent desire to prevent the extension of slavery. Berwanger is careful throughout to indicate his awareness that not everyone opposed to slavery extension was so for reasons of prejudice, and he candidly remarks, late in the book, that "the exact extent of racial prejudices as a factor encouraging the limitation of slavery is indeterminable." Certainly, on the basis of the evidence presented here, one is forced to agree.

Cornell University JOEL H. SILBEY

MOST UNCOMMON JACKSONIANS: THE RADICAL LEADERS OF THE EARLY LABOR MOVEMENT. By *Edward Pessen*. ([Albany:] State University of New York Press; distrib. by Antioch Press, Yellow Springs, Ohio. 1967. Pp. viii, 208. $7.00.)

To his continuing study of the role of the labor movement during the Jacksonian period, Professor Edward Pessen has added his latest contribution, *Most Uncommon Jacksonians*. In this volume, after briefly discussing the Workingmen's parties and the trade unions of the period, Pessen chooses a number of their leaders for his study—namely, Thomas Skidmore, Robert Dale Owen, George Henry Evans, Stephen Simpson, Thomas Brothers, John Ferral, Seth Luther, John Commerford, William English, Charles Douglas, Theophilus Fisk, Ely Moore, and Levi D. Slamm. Pessen briefly describes their backgrounds and their labor careers, but the main focus of his analysis concerns their social philosophies and beliefs.

Believing that the similarities between the ideas of trade-union leaders and those of the political leaders were greater than their differences, Pessen combines all of their views and analyzes them, as "children of the enlightenment." He examines their views concerning the nature of American society, their criticisms of Malthus, their conclusions concerning the causes of social distress, and their view of "the good society and how to achieve it." He then concludes this short and well-written book by justifying his description of them as radical in that they believed that America was "torn with social conflict, disfigured by the misery of the masses, and dominated by the greedy elite . . . a society whose transformation in the future depended on the independent action of labor. . . ."

Pessen is convinced that these leaders were radical if that term has any meaning. I, too, think it ought to have meaning; as defined in the dictionary, "radicals" are a group "favoring fundamental or extreme change." Now, Skidmore may fall into this category, and, to a lesser degree, so do a few of the others.

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But many of them favored a vague and unidentified "new society in which labor's reward was at least equal to that of all other groups, both in status and income" —a platitude that nonradicals could support. And if it is difficult to ascertain where, in their ideas, some of the labor leaders proposed "fundamental or extreme changes," one would find it even more difficult to prove them radical from what most of them did in practice. Even Pessen agrees that they were pragmatists, who really did very little except favor some vague reforms, mainly in education, rather than fight for any fundamental changes in American society.

Pessen justifiably refuses to study the ideas of those labor leaders who did not articulate their thoughts "since the social ideas of silent men" are impossible to discuss. But their actions, even though requiring more work to discover, may be more relevant to the history of American labor than the social ideas of some who speak, but do not practice.

Michigan State University ALBERT A. BLUM

AUGUST BELMONT: A POLITICAL BIOGRAPHY. By *Irving Katz*. (New York: Columbia University Press. 1968. Pp. xiv, 296. $10.00.)

AUGUST Belmont, successful international banker and chairman of the Democratic national committee from 1860 to 1872, has had to wait until now for a full-length biography because, as Irving Katz explains, "the files of the Wall Street banking firm, August Belmont & Company, together with much of Belmont's political correspondence, were destroyed by the Equitable Building fire in 1912." Accepting the loss of the business records as irreparable, Katz has managed to piece together Belmont's political career by means of diligent research in the published sources, in letters preserved by descendants of Belmont, and in the manuscript collections of his contemporaries. "Gaps still remain in Belmont's story," as Katz remarks. Yet this book affords new perspective on presidential campaign politics from Franklin Pierce to Grover Cleveland.

It is, however, limited perspective, for Belmont, despite his wealth and influence, was hardly a major figure in the presidential wars, and Katz's tight-woven account of his political opinions and activities does not materially alter the standard interpretations of well-known events. What it does, essentially, is to add worthwhile detail to our knowledge of certain events, as, for example, Civil War diplomacy and finance, the Liberal Republican movement, and the Hayes-Tilden electoral crisis. Katz also shows how, as national chairman, Belmont helped to preserve the Democratic party from oblivion after it was fragmented by secession and civil war. After Cleveland's election in 1884, he tells us, the leading member of the Democratic national committee, Senator Arthur P. Gorman of Maryland, wrote Belmont that the victory "'would not have been possible but for the foundation laid by you.'" And Katz concludes: "I believe Gorman was correct; his statement was not one of ceremonial rhetoric but an accurate evaluation."

On the whole, Katz's treatment of Belmont is markedly objective. He shows him as an ambitious, aggressive banker-politician who suffered from excessive ego, occasionally bent the truth, cared little for the Negro or the white laborer, thought gold the only sound money, and combined staunch loyalty to America

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and the Democratic party with unfulfilled desire for a choice diplomatic post as a reward for his political services. Notwithstanding a number of rough spots in style and several places where more background is needed, this is a readable book. The numerous illustrations are interesting. On page 213, mixed captions identify Carl Schurz as Samuel J. Tilden, and Tilden as Schurz.

University of California, Riverside HAL BRIDGES

THE GERMAN-AMERICANS: AN INFORMAL HISTORY. By *Richard O'Connor*. (Boston: Little, Brown and Company. 1968. Pp. xii, 484. $8.95.)

RICHARD O'Connor's "informal" history of the Germans in the United States is an account of the experiences and contributions of the German element in the building of America, in politics, industrial progress, art, music, and other cultural activities, from colonial times to the present. The early chapters are largely based on A. B. Faust's two-volume history of the German element in America. Most of the book, however, deals with the nineteenth and twentieth centuries. We get the familiar story of the impact of the refugees of the German Revolution of 1848 and the reaction of American nativists to the radicals, socialists, and freethinkers, who constituted an important minority among the total immigration of the middle 1850's and made notable contributions to American politics and culture. Puritanism clashed with the European "Continental Sunday." In politics the Germans opposed woman suffrage and prohibition, and many of the newcomers played a significant role in the crusade against slavery and found a home in the new Republican party. A list of prominent Germans in America is a long one, from all walks of life, in art, music, the theater, and other cultural activities. The list would include Americans of German ancestry, from Eisenhower to "Babe" Ruth, "Honus" Wagner, Lawrence Welk, and scientists and engineers like C. P. Steinmetz and J. A. Roebling. A whole chapter is devoted to "Carl Schurz, the Good, Good German." Another chapter deals with social history and has the title "Down Where the Wurzburger Flows." Other chapters describe the German immigrants in the westward movement, including the "Latin farmers" who could read the classics in the original, but knew very little about plowing a field.

The pages devoted to the impact of two world wars on America's Germans are of major interest. The loyalty of the Germans to their adopted country involved breaking their emotional ties with the fatherland. The debates in World War I were acrimonious and bitter. Propaganda, neutral rights, the drift from American neutrality to actual participation in a shooting war of brother against brother produced a tragic crisis among the Germans that many of their American neighbors did not understand. Overheated patriots set out to eradicate everything of German origin, including the "hyphen" of German-Americans. While, for the most part, the Germans met the standard of loyalty to the United States, World War I destroyed America's "Deutschtum," and it was never to be revived.

The author, the great-grandson of a Hessian, has maintained commendable objectivity in dealing with these unhappy events. In his own words, he has written an "informal" history—whatever that means—and not a "scholarly exercise." That task he assigns to "academic professional historians and social scientists." The bibliography does not list some important books and articles. Little

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use has been made of German newspapers or of *Der Deutsche Pionier,* published by H. Ratterman in Cincinnati. To give but one example, the Amana settlements in Iowa are discussed without referring, either in the bibliography or in the footnotes, to Bertha H. Schambaugh's book. There are other omissions and deviations from the rigid standards of the guild of professional historians, but they are of no great importance. The book highlights the major events in the history of German immigration in a style that makes interesting reading.

Western Reserve University CARL WITTKE

DIGGING FOR GOLD—WITHOUT A SHOVEL: THE LETTERS OF DANIEL WADSWORTH COIT FROM MEXICO CITY TO SAN FRANCISCO, 1848–1851. Edited, with an introduction by *George P. Hammond.* ([Denver:] Old West Publishing Company. 1967. Pp. 116. $22.50.)

IT is something of a relief to read a collection of letters set in the time of and, to a substantial extent in the locale of, the California gold rush in which the writer never wields a pick or tilts a pan. Instead, Daniel Wadsworth Coit, uncle of Daniel Coit Gilman, successively president of the University of California and of Johns Hopkins University, came to California from Mexico at the age of sixty-one for the purpose of supplying Californians with gold and silver coin in exchange for gold dust which he would sell at a healthy commission. Daniel Coit made his first fortune in Chile and Peru as a partner-representative of a German business firm. In 1828, at the age of forty-one, he shed the pressures of business, traveled in Europe, returned to Connecticut to marry his second cousin Harriet Frances Coit, settled on an estate not far from New York, and then, when the depression of 1837 carried off most of his resources, he moved with his wife and children to his parental home in Norwich, Connecticut, where he developed a reputation for producing select fruits and vegetables. In 1848, Coit was called from these pastoral pursuits to go to Mexico as confidential agent for a New York firm, managed by cousins and friends, that was shipping supplies to the United States Army during the Mexican War.

About half of the fifty-two letters printed herein, all written to his wife, are from Mexico City, and they describe the essentially placid and graceful life of an experienced and conservative businessman moving within the higher echelon of the Mexican business community. Much of Coit's free time was given to indulging his artistic taste and talent, and his sketches of cathedrals, convents, street scenes, and country estates are unpretentious and pleasant to look at. After reaching San Francisco by steamer from San Blas on April 1, 1849, Coit tended strictly to his business of trading, investing, and buying gold, keeping religiously aloof from all mining operations. Commenting on the misfortune of a Bostonian who had to borrow the price of passage home, Coit epitomized the gold rush experience in a shrewd phrase: "This is the scheme of the great California lottery—a few brilliant prizes, many smaller ones, but the greater portion blanks." The Connecticut Yankee revealed by these letters was judicious, literate, cautiously opportunistic, and proper almost to the point of prudery.

In a competent introduction George P. Hammond provides helpful and appropriate historical and biographical background for these letters, and he exer-

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cises commendable restraint in editing them. The book is handsome in design and printing, as it ought to be for the price.

University of Oregon EDWIN R. BINGHAM

NEW MEXICO IN 1850: A MILITARY VIEW. By *George Archibald McCall.* Edited and with an introduction by *Robert W. Frazer.* (Norman: University of Oklahoma Press. 1968. Pp. xi, 222. $5.95.)

WHEN the United States acquired New Mexico in 1848, Americans knew very little about the region, and the army, upon whom so much of the initial occupation depended, lacked detailed information on which to base its policies and its operations. To meet this need the War Department commissioned Major George Archibald McCall, of the Third Infantry, as a colonel in the Inspector General's Department and directed him to inspect the military Department of New Mexico. In addition he was to report on the general political, social, and economic conditions in the new territory. McCall's "Report on Conditions in New Mexico," dated at Santa Fe on July 15, 1850, his inspection reports of the army posts in September and October, and a "Report on the Defensive Needs of New Mexico," written on December 26, 1850, after his return to Philadelphia, form the core of this book.

The documents present a valuable picture of New Mexico at the time it passed into American control. McCall was an intelligent and perceptive observer, and his "Report on Conditions" is full of details on the character of the population, on the economic possibilities of the region, which he listed in order of importance as mining, stock raising, and agriculture, and on the dangers from the wild Indians. The inspection reports, following as they do the narrow prescriptions of the army regulations, are prosaic and less interesting, but they indicate the importance of the army in New Mexico and mirror the difficulties of supplying troops and horses in a distant land that could supply little but coarse and gritty wheat flour and inadequate fodder.

The documents presented in the book seem almost too slight to bear the heavy weight of introduction and notes that the editor has thrown around them. Frazer's work, however, is competently done, and his seventy-five pages of introductory material skillfully set the stage for McCall's official reports.

Marquette University FRANCIS PAUL PRUCHA

THE AUSTRALIAN BALLOT: THE STORY OF AN AMERICAN REFORM. By *L. E. Fredman.* ([East Lansing:] Michigan State University Press. 1968. Pp. xii, 150. $5.75.)

BETWEEN 1888 and 1892 thirty-eight states adopted some form of the Australian ballot. Historians have not been impervious to the importance of the Australian ballot, but this unimaginative volume is the first book-length discussion since 1917 of the origins, development, and significance of the reform movement that transformed American electoral legislation.

The contributions of the book are desultory but real. The author is at his best when he identifies the individuals (Henry George, Richard Henry Dana III,

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George Walthew, and Robert Schilling) and the groups (mugwumps and single taxers, but not the Populists) who were most responsible for ballot reform in the late nineteenth century. Also significant are the investigation of the origins of ballot reform in Australia and Great Britain and the attempt to limn the relationship between Australian and British reformers and Americans who sought new electoral laws. Especially pertinent is the use of Australian newspapers to discover the motives of ballot reformers in that country. Finally, the author correctly notes that a concern for electoral machinery is part of a "continuing inheritance" that connects mugwumps to progressives.

Unfortunately, the weaknesses of the book are also numerous. Like many authors, Mr. Fredman claims too much for his subject when he declares that "the quest for the Australian ballot . . . stimulated other reforms which led to the heart of the Progressive era." Moreover, the last chapter, which asks whether the Australian ballot has successfully contained corruption in the twentieth century, while it is necessarily cursory, is unnecessarily superficial. Indeed, the treatment often becomes superficial when the author digresses from the immediate subject of the Australian ballot to discuss such varied topics as the nature of boss rule and the importance of the cross-filing law in California politics. Such provocative topics as the debate over compulsory voting are raised, but not pursued. Finally, the traditional methodology and repetitive organization add little luster to the book.

Some of the ingredients of a fine study are here, but the book needs more polish.

Drew University J. Perry Leavell, Jr.

"THE BEST MEN": LIBERAL REFORMERS IN THE GILDED AGE. By *John G. Sproat.* (New York: Oxford University Press. 1968. Pp. ix, 356. $7.50.)

The mugwumps have won praise from historians who accepted their strictures against late nineteenth-century society. This book challenges both that indictment of society and mugwump beliefs. Scholars can no longer laud "independents" for opposing alleged corruption without recognizing that they hated coalition party government in a pluralistic society and that they were elitists who bitterly opposed the democratizing tendencies of their time. Mugwumps earnestly thought "the best men"—E. L. Godkin, G. W. Curtis, and the ubiquitous Adamses—should rule and set the social tone. Educated in classical subject matter, they drew moralistic beliefs from a preindustrial, hierarchical world. They eagerly accepted fixed natural laws that supposedly governed both private conduct and the public market place.

Professor Sproat demolishes these views nicely. Mugwumps held that political independence added purity to democratic politics. But they expected rewards for writing and speaking, as nasty politicians of both parties discovered. They wanted a small, technically efficient government, with themselves in charge, reducing property taxes, encouraging individualism, and curtailing public services. Behind this thin shield, each man used his talents, benefiting society without warping nature's laws. And men with talent would always rise, no matter what

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the circumstances of their birth or living conditions. This comfortable determinism, which they could afford, let mugwumps rationalize away their refusal to help Negroes, immigrants, or organized labor.

Sproat describes men and political issues trenchantly. Mugwumps wanted gold currency because God apparently made the metal to ensure price stability for property owners. They opposed the tariff because it brought government into economics to help labor and business. Sproat cuts short their cant about protection: "The tariff may have been a blood relative to the trusts, but it was not their mother." Mugwumps supported government action against labor unions without surrendering any philosophical purity, but usually opposed regulating business.

The author is not always correct in delineating the mugwumps' opposition. His remarks on President Arthur, Mark Hanna, and business influence in politics are debatable. But he sees clearly the miscasting of textbook heroes and villains. His dislike of Grover Cleveland, the period's chief spokesman of remorseless laissez faire, is refreshing and appropriate. He notes Cleveland's shrewd use of machine bosses and ultraconservative business elements to promote his own career, while at the same time he was protesting volubly about disinterested public service. Sproat also sees James G. Blaine for what he was, a brilliant party leader and constructive moderate. The mugwumps hated Blaine because he genuinely represented common men and coalition politics, not for any personal corruption. Sproat is not yet ready to see that politics and society were not especially irresponsible in this era, but his insights help fortify that conclusion.

The mugwumps' sharp pens and tart tongues enhanced their historical reputations. But Sproat is obviously impatient with rhetoric: "Unable to come to terms with his age, the liberal reformer exaggerated its defects and overrated the past." Sproat writes with pungent clarity, and his work is well organized around original research; it is an outstanding book and a welcome addition to the current re-examination of the Gilded Age. In helping to shift the emphasis from those who talked to those who acted, it strongly suggests, as many recent works do, that a new view of the period is much overdue.

University of Texas, Austin H. Wayne Morgan

THORSTEIN VEBLEN: THE CARLETON COLLEGE VEBLEN SEMINAR ESSAYS. Edited by *Carlton C. Qualey*. (New York: Columbia University Press. 1968. Pp. ix, 170. $6.00.)

The papers in this symposium were presented to a special Veblen seminar at Carleton College in 1966, when the institution was celebrating its centenary and honoring a famous alumnus. Not all readers will accept Professor Qualey's contention that Veblen was "America's greatest social critic of the first half of the twentieth century," but it is a useful starting point.

The authors are more interested in Veblen's social thinking than in his formal economics. Charles B. Friday follows two themes in Veblen's writings: his search for "the goal of maximum production," and his "dislike of institutionally imposed or created inequality." Thomas Cochran's "Business in Veblen's America" is a lucid, balanced essay. He sees shortcomings in Veblen's predictions about capitalism and doubts the merits of a society run by experts and engi-

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neers, which Veblen apparently wanted. Veblen was wrong in thinking that businessmen would curtail production to maintain price levels. And, like most contemporary critics, he overestimated the role of business in politics. David W. Noble's essay on Veblen and American theology is at once the most interesting and most difficult in the book. His efforts to link Veblen to Puritanism and the search for a lost Eden are stimulating but inconclusive. Noble might profit from contrasting Veblen's intriguing ideas about man's origins and social nature to those of Sigmund Freud. Joseph Dorfman's essay, "Background of Veblen's Thought," is comfortably shallow, with no special insight into Veblen's times. The last chapter contains some engaging recollections by Isador Lubin, a Veblen student at the University of Missouri. Veblen's dry front hid a sharp tongue. A remark after seeing *Birth of a Nation* is classic: "Lubin, that is the finest example of concentrated misinformation that I've ever seen." Lubin notes that Veblen did not overtly dislike the system he criticized, but he was a detached social observer. Lubin also thinks that Veblen underestimated capitalism's ability to change. These essays are seldom innovative, but they are a helpful beginning in delineating Thorstein Veblen's thought.

University of Texas, Austin H. Wayne Morgan

THE EIGHTEENTH MISSOURI. By *Leslie Anders*. (Indianapolis: Bobbs-Merrill Company. 1968. Pp. xi, 404. $5.95.)

The Eighteenth Missouri was a regiment of Civil War volunteers who participated in a remarkable range of military operations. Involved initially in the bitterness of borderland dissension, the regiment soon saw action at Shiloh, where, in the thick of battle, many of its men were captured, and they experienced life in a Confederate prison until paroled. Occupation duty in northern Mississippi brought exposure to sporadic sniping and some pitched battles. After the relative lull during the winter of 1862–1863, the regiment participated in the operations culminating in the Battle of Chattanooga. Thereafter, sometimes committed in the heaviest fighting, the Eighteenth was with Sherman all the way to North Carolina. Few Federal regiments could provide as panoramic a screen for portraying Civil War experiences.

Professor Anders has sought to include as much of the experiences of these men as his sources will yield. Marching, camping, and fighting, suffering and dying, sweating to destroy or rebuild railroads, and playing pranks or brawling fill only part of these pages. Also intruding into the regiment's affairs were the personal and political interests of the Missouri governor, the craving for military patronage of other politicians, and the favoritisms or even political ambitions of many generals. And there was always the news from home, too often bringing word of heartbreak in a private world maddeningly beyond the reach of these men.

The author forcefully states his thesis that unit histories are now needed more than either campaign-level or biographical accounts. With painstaking attention to reader appeal, the author has used a very informal style to weave a compelling tale from the surviving sources, and it stands as an effective exhibit in support of his thesis. Unfortunately, no essay on sources is included, so that the methods of

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creating this genre are less explicit than would have been expected from so strong an advocate. From the notes it is evident that the recapturing of intimate details had to depend very substantially on reminiscences long after the events. One example is the citing of the Unionville (Missouri) *Republican* of the 1890's more than one hundred times. The author has chosen to weave in without comment much fanciful fabrication, many tales grown taller with the passing years, and a few stories so startling that even he enters a caveat. Also startling is the appearance of Birmingham, Alabama, which was not founded until the next decade, on two war theater maps and in the text. Granting the limitations imposed by the nature of the sources, this story of the Eighteenth Missouri is rich ore for understanding what it was like to serve throughout the war in a Federal combat unit.

University of Alabama THOMAS B. ALEXANDER

FOR THE UNION: OHIO LEADERS IN THE CIVIL WAR. Edited by *Kenneth W. Wheeler.* ([Columbus:] Ohio State University Press. 1968. Pp. viii, 497. $10.00.)

THIS volume of nine essays, edited by a former consultant to the Ohio Civil War Centennial Commission, is a useful contribution to Ohio and Civil War history, but, as is usual with such collections, the essays vary in their value.

Frank L. Klement's long essay on Clement L. Vallandigham is well done, but presents little that is new. Allan Peskin, writing on James A. Garfield's military career, stresses his dislike of West Pointers, praises his work as Chief of Staff for General W. S. Rosecrans in 1863, and states that of all Ohio generals he "came closest to personifying . . . the political general." Two essays give a good picture of wartime newspaper reporting. Robert H. Jones describes the work of youthful Whitelaw Reid of the Cincinnati *Daily Gazette,* and Donald W. Curl that of Murat Halstead of the Cincinnati *Commercial.* Both journalists were sharply critical of military officers and of the conduct of the war in general. Some of Halstead's rivals dubbed him "Field Marshal." Reid's bitter criticism of officers and men at Shiloh was partly responsible for General Halleck's order forbidding newspapermen to accompany his army to Corinth.

Mary Land, writing on Senator Ben Wade, emphasizes his dissatisfaction with President Lincoln and his policies, his desire for a more vigorous prosecution of the war, and his work as chairman of the Joint Committee on the Conduct of the War—work that she presents in a favorable light. James B. Bell's essay on Charles P. McIlvaine, bishop of the Protestant Episcopal Church who spent six months in England at the request of the State Department, does not alter the story of Anglo-American relations during the war. Miles Greenwood, one of the most important ironmasters of the West, is the subject of Carl M. Becker's essay. It considerably illuminates supply problems of the federal government, but unfortunately only one-third of the essay is on the war period. Greenwood's iron works "modernized" muskets, produced cannon, gun carriages and related equipment, and many small articles. It also built one monitor which, however, was not completed until after the war had ended.

The late Harvey Wish wrote of Artemus Ward (Charles F. Browne) and Petroleum V. Nasby (David Ross Locke)—two of Lincoln's favorite humorists.

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Their writings, Lincoln said, were medicine that gave him relief from the strain of his work. Wish calls the two men "morale-builders."

The essay on John Sherman by Jeannette P. Nichols reflects her long-time research on Sherman and is the most valuable one in the volume. She concentrates on his wartime career and concludes that he was "a leader who as statesman and politician was sometimes wise, sometimes foolish, sometimes blind, sometimes farseeing."

The volume has a few factual and a few typographical errors, but it is relatively free from such.

University of California, Los Angeles BRAINERD DYER

HISTORY OF ANDERSONVILLE PRISON. By *Ovid L. Futch.* ([Gainesville:] University of Florida Press. 1968. Pp. v, 146. $5.00.)

THERE is no more poignant yet ghastly chapter in the history of the American Civil War than that dealing with prisons and prisoners of war. The vast literature on this complex subject is emotional and conflicting, and it tends toward extremes. To master it with any degree of objectivity calls for continuous application of the strictest canons of historical methodology.

Until now, with the possible exception of William B. Hesseltine's excellent *Civil War Prisons,* there has been little successful effort made to depict in a scholarly, balanced fashion the management, conditions, and policies of perhaps the most terrible of all Civil War prisons—Andersonville. Into this sixteen-and-one-half-acre compound in southwestern Georgia were crammed as many as 31,000 Union prisoners. The mortality rate rose alarmingly there, reaching 100 deaths per day for the month of August 1864. Well over 13,000 men in blue perished in this putrid stockade. In this book the late Ovid L. Futch comes close in many ways to satisfying the need for a scholarly, sober study of Andersonville.

Any work that must deal with such an appalling subject, involving depravity, crying shortages, unspeakable filth and degradation, and heart-rending mismanagement, faces a stern challenge. But the author has consulted a large array of fugitive primary source material in manuscript form as well as the standard printed documents and personal narratives. And in the breadth and depth of his research, in his notable impartiality, and in his clumsily written yet thorough descriptions of the fearful scene at Andersonville, Futch has succeeded well.

On the debit side, however, in addition to the graceless, stilted writing style, the author is repetitive, fails in smooth transition from one paragraph, topic, and chapter to another, and organizes his material poorly. Matter obviously belonging in one chapter (such as in "Prison Conditions") suddenly appears in another (as in the one on prison management). And while Futch handles the controversial Confederate prison supervisor, General John H. Winder, capably and provides a sound chapter on the murderous Federal inmates known as the "Andersonville Raiders," the good descriptive parts of the book are not buttressed properly with essential analyses or assessments. For example, with respect to the enigmatic southern prison keeper at Andersonville, Major Henry Wirz, Futch fails completely to come to grips with him or to evaluate this pivotal figure in any kind of depth.

Yet, on balance, this slender little volume marks an advance in Civil War

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writing concerned with the heated and melancholy issue of prisoners of war and especially of the most notorious of all prisons, Andersonville.

Pennsylvania State University WARREN W. HASSLER, JR.

THE GETTYSBURG CAMPAIGN: A STUDY IN COMMAND. By *Edwin B. Coddington.* (New York: Charles Scribner's Sons. 1968. Pp. xviii, 866. $15.00.)

THE achievement of this book is that it pierces the fog of battle at Gettysburg so far as the tactical handling of troops is concerned. The order of battle is pursued down to the regiments and batteries with such tenacity and thoroughness that no one is likely ever to do it again. This in itself is enough to establish *The Gettysburg Campaign* as a labor of love as well as a model of the most painstakingly orthodox military historical scholarship.

Yet for all of that something is awry. Fundamentally, it is the derivative subject of the book, *A Study in Command,* which is offered as the justification for doing Gettysburg again. But "command" is a difficult conception. As it takes its operational form it is a compound of social imperatives and personal styles in collision with events—a society militant, as it were, mediated by its generals. And for those who would undertake to exploit the intriguing possibilities of command conceived in this way, narrative must give way to analysis and interpretation. Correlli Barnett's *The Swordbearers* offers a splendid example of what might be accomplished along this line.

Professor Coddington, on the other hand, has approached command much more conventionally and less rewardingly. While he does not offer a definition of his subject, it seems to mean the simple and literal transmission of intent to orders to execution, and his book is essentially the narrative of how this process took place on the tactical level. Yet even within this framework there are difficulties. The subject is self-limiting, but there are an extraneous section on infantry weapons and a digressive, though illuminating and suggestive, chapter on the "plunder" of southern Pennsylvania in which the prosecution rather unwittingly makes the case for the defense. As Lee's army went about its strategic assignment of living off the countryside, it stayed so far within Lee's orders for procedural treatment of enemy noncombatants and property that the few violations cited make one marvel at Confederate supervision and control. But when Coddington stays within the self-imposed limits of his subject, he handles an exceedingly murky business with clarity and restraint and sometimes with superb distinction. His description of the intent and execution of Pickett's charge invites favorable comparison with Freeman's *R. E. Lee.*

The commanders fill the book. In the forefront are Meade, Lee, Stuart, Pleasanton, Longstreet, Hooker, and Sickles (the villains of the piece), Reynolds, Warren, Hancock, Early, and Ewell; in the background hover Lincoln and Halleck, in contradistinction to Jefferson Davis who all but vanishes. Then there are the less than great, the regimental commanders who brought "command" to the last meaningful tactical level. With few exceptions the commanders as personalities cease to exist; they become only names, either doing or not doing what they were ordered.

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Yet, paradoxically, this is a book about a man—George Gordon Meade. It is his defense and extenuation for failing to crush Lee once he had stopped him. This necessitates opening the account at Chancellorsville so that Lee might find the way open for his thrust north and Hooker might be found wanting, thus preparing the way for Meade. In disposing of Hooker, Coddington uses a twin thesis: that he lost his nerve and that he made no serious attempt to formulate a plan, preferring instead to wait and let events unfold and decide for him. No doubt he is correct on both counts though his summary judgment of Hooker threatens him with that endemic temptation of military historians: the desire to have it both ways. For when Meade subscribed, as he had to, to the Hooker idea of having no plan until Lee's intentions might be gauged, no condemnation falls on his head.

Of course judgment must be made, but this is always risky. At one point Longstreet is chided for going on the line with one of his divisions—"many . . . generals were given to displays of this sort"—instead of staying in his headquarters from which he could supervise his corps. Later, however, Richard Anderson is harshly judged for having a division headquarters and staying at it, instead of going down to his brigades. Longstreet is then adduced and given good marks for having done the reverse. If one were looking for a principle of command, it apparently would be what is sauce for a major general is not always sauce for a lieutenant general. In short, all things being fairly equal—and Coddington is at great pains to equalize the striking power of the two armies, thereby increasing Meade's achievement—it depends upon where one's sympathies lie and how things turned out. What is correct is what worked; what is incorrect is what failed. Schools' solution doctrine has a way of disintegrating on the battlefield.

Judgments on the commanders abound in this book. Their merit lies less in their subtlety or insight than in the searching, exhaustive inquiry upon which they are based. The results are often challenges and correctives to many of the partisan cases that have been made about Gettysburg. Coddington's own gentle, thoughtful partiality for Meade and the Army of the Potomac is less of a forensic position than a sign of the tempered judiciousness that informs the entire book.

University of North Carolina, Chapel Hill PETER F. WALKER

THE SECOND REBELLION: THE STORY OF THE NEW YORK CITY DRAFT RIOTS OF 1863. By *James McCague*. (New York: Dial Press. 1968. Pp. viii, 210. $5.95.)

It began on the hot Monday morning of July 13, 1863, when an angry, impatient crowd that had gathered for the calling up of drafted men (the first such conscription in the nation's history) suddenly exploded and quickly wrecked and burned the draft office. Mobs spilled into the streets, and for three days and nights they looted, burned, beat, and murdered from the Bowery to Harlem. They destroyed telegraph lines, seized weapons, ripped up railroad ties, attacked police, military, and Negroes many of whom were hanged from lampposts. Uncounted thousands of rioters were wounded, and many were killed. For these few days the Civil War was transferred to urban New York where the discontented struck against Lincoln and war, the Negro, the rich, and authority. This was in many

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ways, as Mr. McCague ably describes, a second rebellion which, at least momentarily, distracted a nation elated over victories at Gettysburg and Vicksburg.

The author has carefully culled much of the available secondary literature and has digested a good portion of the newspaper sources from which he presents a clear account—a difficult task in view of the confusion of the day. Yet the book falls short of being an incisive effort and adds almost nothing to the knowledge of causes and effect. While the sensationalism of the event is headlined, little is done to analyze New York society, which was traditionally segregated and southern-oriented. A discussion, even without great detail, of class antagonisms between labor resentful over inflationary prices and low wages and the politics of the new "shoddy" millionaire would have been helpful. Would the riots have occurred even without the war? Such basic information as a survey of population, ethnic divisions, the number and position of the immigrant, and the political divisions of the city is not related. McCague would have done well also to consult important primary material including the records of the Department of Finance and Comptroller's Office and those of the Court of Oyer and Terminer where many of the rioters were tried and where a number (not just two as the author states) were found guilty of murder. McCague may be right in sensing some peculiar relationship between politicians, possible conspirators, and the mob, but he presents little to substantiate this theory. Perhaps no amount of research will entirely raise the secret veil of rebellion, but the search should be more carefully and diligently pursued than has been done here. This is a well-written and very detailed volume, and surely the additional effort would have been most worthwhile.

Queens College, Flushing, New York LEO HERSHKOWITZ

PLAINS INDIAN RAIDERS: THE FINAL PHASES OF WARFARE FROM THE ARKANSAS TO THE RED RIVER. By *Wilbur Sturtevant Nye.* (Norman: University of Oklahoma Press. 1968. Pp. xx, 418. $9.50.)

IN the early 1930's, Colonel Wilbur S. Nye drew an official assignment to prepare a history of Fort Sill, Oklahoma. He spent four years researching official documents and conducting extensive interviews with aged Kiowa and Comanche veterans of the Plains Indian wars. The result, *Carbine and Lance,* was published in 1937. Through three decades it has remained one of the very best books about the Indian hostilities of the southern Plains because it offers rare insight into both army and Indian attitudes, motivations, and relationships.

In *Carbine and Lance,* Nye dealt only briefly with the period of conflict before the establishment of Fort Sill in 1869. Now in *Plains Indian Raiders* he treats the hostilities of 1864–1869. The warfare along the Smoky Hill Trail in 1864–1865, the Hancock campaign of 1867, the Medicine Lodge Treaty and its legacy, and Sheridan's winter operations of 1868–1869 constitute most of the narrative. It is well done, as was to be expected, but it does not display the keen understanding and felicity of expression that marked *Carbine and Lance.*

Nye had another, not altogether compatible, objective: to tell the story of the photographer William S. Soule and to reproduce a nearly complete collection of Soule's superb Indian portraits. Not much is known about Soule, and what Nye has learned serves mainly to disrupt the unity of a narrative dealing with larger

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events. But the photographs—more than a hundred by Soule and a few by other photographers and illustrators—afford a remarkable visual record of the tribal leaders of the southern Plains in the 1860's and 1870's. Although many have been published before, and a competing volume is now reaching the market, never have so many of Soule's Indian views been included in a single presentation.

Even though less satisfying than *Carbine and Lance, Plains Indian Raiders* is a useful companion, and it has the added attraction of bringing together the fruits of a fine photographer's productive years on the Plains.

National Park Service ROBERT M. UTLEY

PURITY IN PRINT: THE VICE-SOCIETY MOVEMENT AND BOOK CENSORSHIP IN AMERICA. By *Paul S. Boyer.* (New York: Charles Scribner's Sons. 1968. Pp. xxi, 362. $7.95.)

TOCQUEVILLE discovered in 1830 that a chaste and moralistic public opinion in America functioned informally but effectively to suppress licentious and irreligious publications. He reported this in connection with a discussion of the absence of any real freedom of thought in this country. Americans were neither corrupt nor creative; they were pure but uninteresting. It is obvious that sometime subsequent to his visit they discovered both art and sin, and the paths of virtue and beauty seemed to diverge. The anguish of would-be censors occasioned by the growing awareness of this divergence furnishes Paul Boyer with his principal theme.

The major part of censorship activity, namely the suppression of what Boyer calls "hard core" pornography, is not part of his story. This may be the more important matter from the viewpoint of social history, but it is certainly less interesting than attempts to censor works that could be and were defended on grounds of literary merit. The censorship of books, as Boyer defines it, involves the fine line where literary merit and obscenity come together. The attempt to trace such a line, to suppress the obscene without encroaching on the creative artistic spirit, endows the history of book censorship with its capacity to illuminate the history of taste. It also furnishes a chapter in the history of freedom of thought.

Organized censorship activity began in the 1870's with the attempt of wealthy and influential citizens to curb the licentious excesses of the urban masses. Vice societies were formed in major cities, and those in New York and Boston receive principal attention. Their officers—Comstock, Sumner, Chase, Allen—came to enjoy enough autonomy to direct activity not only against hard-core pornography but against serious literary work as well. As literary naturalism with its more candid treatment of sexual themes succeeded genteel realism, the vice society censors arranged a direct confrontation between the traditional moral sense of the community and its literary elite. This was certainly a mistake of the first magnitude. Wealthy sponsors quietly dropped out; timid publishers were transformed into bold champions of a free press; legislatures were pressured to reject tighter obscenity laws; finally, the courts so construed obscenity statutes as to exempt works of responsible intent. Thus isolated, the vice societies withered and died.

As one would expect, the author treats the censors with a distinct lack of sympathy. But in his introduction, which was apparently written sometime after the body of the text, and which is properly an afterword, he notes the sober second

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thoughts of several contemporary writers and critics who have begun to wonder whether unrestrained freedom of expression may not nurture a callousness that will eventually prove degrading both to society and to literature itself. Boyer has carefully read the sources, interviewed surviving participants of the censorship battles, and has produced an eminently readable and judicious account.

University of Iowa STOW PERSONS

THE ACHIEVEMENT OF WILLIAM DEAN HOWELLS: A REINTERPRETATION. By *Kermit Vanderbilt.* (Princeton, N. J.: Princeton University Press. 1968. Pp. ix, 226. $6.50.)

THE problem of reaching a just estimate of William Dean Howells continues to vex literary critics and historians. What is to be done with this prolific author who had so many friends and campaigned with such earnestness for American "realism," whose "beautiful time" always seems about to come?

Mr. Vanderbilt's solution is a detailed biographical and historical examination of four novels from the 1880's. Three of them, *A Modern Instance, The Rise of Silas Lapham,* and *A Hazard of New Fortunes,* are no surprise, but the fourth (and first to be examined) is the little-known treatment of a Boston spiritualist, *The Undiscovered Country* (1880). Together, Vanderbilt argues, they are Howells' sensitive diagnosis of the private and social ills of America after the Civil War. In *The Undiscovered Country* Howells writes of religious and moral uncertainty, sexual repression, and rural decay. Related pictures of father-daughter complexes and the dying village appear in *A Modern Instance,* along with mild satire on upper-class Boston. *Silas Lapham,* besides being a drama of the new manufacturer wrestling with his soul, is even more interesting to Vanderbilt for its glimpses of Howells' anxiety over crowding immigrants and rising Jews. Piecing together the novels, letters, and other writings, he indicates that Howells shared the racial fidgets of Brahmins like Lowell, Norton, and Holmes, Sr., even though politely opposing them about literature. Thus, according to Vanderbilt, Howells' attempted resolution of these and other social questions in *A Hazard of New Fortunes* is extremely complex and agonized.

It is not established, however, that this constitutes an "achievement," at least not in the honorific sense. What really needs to be said is that Howells is more important for his muddle and confusion than for his luminousness, but Vanderbilt is reluctant to approach him that way. He supplies some evidence for such a true reinterpretation, but, in overrating Howells as an artist and being humbled by Howells the conscientious and respectable citizen, he does not take the next steps. In the Electra complexes of Howells' heroines, for example, are themes that reappear in one shape or another in much twentieth-century writing, particularly in work for audiences of the level of Howells' audience. Howells' muddle over the small town, which is treated harshly in his "realism" but is repeatedly an attractive subject in reminiscences, certainly has reasons and connections yet to be traced. When these things are seen clearly, however, it will be the deserved achievement of social historians and Howells' critics, not that of poor, anguished Howells.

University of Iowa ROBERT F. SAYRE

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BOSS COX'S CINCINNATI: URBAN POLITICS IN THE PROGRESSIVE ERA. By *Zane L. Miller.* [The Urban Life in America Series.] (New York: Oxford University Press. 1968. Pp. xii, 301. $7.50.)

THERE is much to recommend in this study of Cincinnati politics and reform in the progressive period. Probing deeply into the sources, Zane L. Miller has uncovered much material that is important for understanding the alterations and tensions undergone by the American city at the turn of the century. His descriptions of the city's changing demographic patterns, the structure and composition of its reform organizations, and its governmental developments and problems are especially penetrating. Miller views Boss Cox as emerging from a "decade of disorder" and imposing a "new order" on the city's politics and government. A city charter providing for a bipartisan board of administration, a renovated police force, and improvements in education, park facilities, and fire protection was typical of the programs supported by Cox and his allies. As Miller shows and as Cox himself noted, a boss was "not necessarily a public enemy." But, Miller writes, many "philopolists" demanded an even more widespread approach to the city's problems. Civic boosters, advocates of the social gospel, philanthropists, and municipal efficiency experts supported programs seeking to provide a "new urban discipline in their mobile and divided new city." Dissatisfied with Cox's leadership, these "spokesmen for the new urban discipline" penetrated into politics and eventually destroyed the Cox regime.

Miller sees as the key to the tensions of the city "the conflicts created by the process of urbanization." Cincinnati's growth produced three major residential areas—the Circle, the Zone, and the Hilltop—each having certain characteristics. The Circle was the old walking city, the area inhabited by the newest urbanities; the Zone was a transitional area; and the Hilltop contained the homes of the city's elite. According to Miller, all were heterogeneous in regard to ethnicity, race, and religion. This heterogeneity leads the author to conclude that "residence rather than race, religion, or ethnicity provides the touchstone to the city's social and political experience."

Many readers may share my skepticism about this statement, particularly considering the frequency with which Miller mentions the importance of such ethnocultural issues as Sunday closing, personal liberty, religion, and nativism in election campaigns. It is in the area of election analysis that Miller is weakest. His own descriptions of Circle, Zone, and Hilltop suggest that these resident categories are too confining a base upon which to judge political behavior. Most urban sociologists today have discarded the ecological approach and see residence as a reflection of ethnic, religious, racial, and class factors rather than as an independent variable. My skepticism was increased by discrepancies between the sources and Miller's discussion of the ethnic composition of the Hilltop area. Miller states that "In 1910 native whites of native parentage constituted over 50 per cent of the population in only one of the four Hilltop wards, the First." But, according to the Thirteenth US Census (although the paragraph is not footnoted, Miller appears to have used this census as the source for his data), native whites of native parentage constituted a clear majority of the First Ward (60.6 per cent) and a bare majority of the Second (50.4 per cent), also a Hilltop ward. What is more

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serious, Miller notes that "Native whites of foreign parentage, most of them German or Irish, made up the largest single element in all four [Hilltop] wards." The Thirteenth Census shows, however, that native whites of native parentage (38,972) rather than native whites of foreign parentage (29,081) comprised the largest single group in the Hilltop wards. The Hilltop would thus appear to have a different ethnic composition than Miller assumes, conceivably challenging his political analysis and modifying his conclusion about the importance of residence over ethnocultural factors in urban politics.

Carnegie-Mellon University JOEL A. TARR

HE WALKED ALONE: A BIOGRAPHY OF JOHN GILBERT WINANT. By *Bernard Bellush*. Foreword by *Allan Nevins*. [Studies in American History, Number 11.] (The Hague: Mouton. 1968. Pp. viii, 246. 30 gls.)

BERNARD Bellush's biography of John Gilbert Winant is a useful chronicle of the career of one who, having been initiated in the Bull Moose campaign of 1912, was destined to spend a lifetime of political frustration working to shape the future of the Republican party while serving the present of the Democratic party. Winant was the product of the kind of upper-middle-class, modest affluence recently acquired by families old enough to move freely in the better social and professional circles. He became, nonetheless, a victim of the ambitions that had motivated the very considerable success of his political life. Suicide ended a career marked by an often remarkable capacity for dealing with frustrations that many men might have pushed aside as products of intransigent historical circumstances but that Winant seemed to internalize, to accept as his own responsibility, and to struggle with calmly, sensitively, and, ultimately, sacrificially.

The biography adds helpful material to the growing literature of New Deal local history through its recounting of Winant's New Hampshire governorship. The details involved in Winant's chairmanship of the Social Security Board and his defense of the program in subsequent political disputes reflect interestingly the dilemma of Republican progressives whose public service efforts to engage themselves in New Deal programs limited their reform effectiveness in their own party. The account of Winant's work in Europe with the International Labor Organization contributes important points about the relation between domestic and emerging international concerns of the New Deal. His succession to Joseph P. Kennedy as ambassador to England and the work of the European Advisory Commission in struggling to manage the politics of European victory press more painful detail into the patternless incredibilia that the closing negotiations of World War II became.

With all of these utilitarian virtues, the book has defects with which readers less interested in the author's careful research and his fortunately full annotation and indexing are not likely to be so patient. Biography is often the only way of introducing the important variations in point of view that the exploration of essentially minor figures can provide, but studies of the periphery of political history are easily overburdened with accumulations of detail more sensitively received by reverential associates who often seem to provide most of them than by the reader whose focus may very understandably fall short of the immediate field. The au-

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thor gives us more evidence than we need of some of the psychological problems Winant faced, but it falls within a range narrowly circumscribed by the affections he inspired. The result is a two-dimensional portrait that aspires to tragedy, but, given the limitations of interpretation and skill with language, it also evokes a pathos that is rarely made to relate either to the achievements or to the failures of Winant's life. From the adoring apostrophe with which Allan Nevins has chosen to open the book to the carefully detailed suicide with which the author closes it, readers will buy many helpful pieces of information at a fairly high price.

Brown University BARRY D. KARL

THE EMERGENCE OF METROPOLITAN AMERICA, 1915–1966. By *Blake McKelvey*. (New Brunswick, N. J.: Rutgers University Press. 1968. Pp. x, 311. $10.00.)

BLAKE McKELVEY's work remains *sui generis;* it invites comparison only with itself. And comparison in this case is inevitable since this latest book begins just where he left off with *The Urbanization of America: 1860–1915*. It would nevertheless be misleading, despite many similarities, to regard one as merely a continuation of the other. The differences in the present volume are important and serve in themselves to measure the changes not only in the urban situation but also in urban studies over the five years that divide it from its predecessor.

Each of the six chapters in *The Emergence of Metropolitan America* covers a single decade, from the Great War through the Great Society. The emphasis is on the growth of metropolitan consciousness, and on political and institutional efforts to find new methods of dealing with the old phenomenon of migration and such attendant problems as housing, employment, and ethnic conflict. There is an abundance of close detail throughout, and in earlier chapters this ranges from the introduction of parking meters to the multiplication of symphony orchestras. But the political emphasis precludes much of the comprehensive description of city life that marked the earlier book; with time the increasing federal involvement in metropolitan affairs tends, perhaps symbolically, to crowd out other topics.

Perhaps the progressive complexity of the urban scene requires this narrowing of focus. The parallel growth of urban studies, however, enables the introduction of a wholly new dimension. The formal "Bibliographical Note" at the end is only a token, a frank admission that any effort is quickly outdated by the rush of new publications. But much of the book itself is a wonderfully comprehensive critical bibliography, a fact that alone assures its importance. Each chapter ends with a survey of contemporary scholarship. Some of this is merely descriptive, but much illuminates the development of the several disciplines concerned, as each of the social sciences has gone about ignoring, modifying, or struggling to incorporate the work of the others. And there is little doubt as to where McKelvey stands on most of the major issues, as he outlines the ideas of the Chicago School in the 1920's, the students of "the power structure" in the 1950's, and Lewis Mumford throughout.

In one sense this bibliographical dimension is not integrated into the rest of the book, as the narrative portions neither use nor perhaps require the more sophis-

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ticated approaches described in the others. In another sense, however, this very lack of integration is a comment on metropolitan history and scholarship. The whole book is actually about several levels of experts. Such formal organizations as the National Conference of Mayors and the National Municipal League dominate those sections not devoted to the planners, architects, and social scientists. If their concerns have not coincided, if much of their activity has been ignored or inadequate, the fault is not McKelvey's. It is instructive to note that each chapter is organized about some dominating event—war, depression, or black migration—that was uncontrolled and often unanticipated by any of those concerned. The author has always been optimistic about the future as well as the quality of life in the metropolis. But this book ends with a description of crisis that "poses a challenge" to "all scholarly disciplines," and to much else besides.

Haverford College ROGER LANE

THE MOONEY CASE. By *Richard H. Frost.* (Stanford, Calif.: Stanford University Press. 1968. Pp. x, 563. $12.50.)

IN this book Richard Frost has provided the definitive history of the famous Mooney case. During the preparedness day parade in San Francisco on July 22, 1916, a bomb exploded among the spectators, killing ten and wounding forty people. Five local radicals were arrested, and two—Tom Mooney and Warren Billings—were found guilty and sentenced, respectively, to death by hanging and life imprisonment. The outcome was suspect from the start. The trials took place in an atmosphere poisoned by hysteria, class animosities, and the irresponsible Hearst press. As Frost demonstrates, the prosecution won its case through suborned and perjured testimony and through the suppression of contradictory evidence. In 1918 Mooney's sentence was commuted to life imprisonment, partly through the intercession of President Wilson. Further efforts on behalf of Mooney and Billings proved unavailing for two decades, however. The Mooney case became a fighting cause for labor, the Left, and civil libertarians across the country. The struggle fell sadly short in the legal sphere. Even after no reasonable doubt remained as to the miscarriage of justice, no legal means could be found for setting aside the decisions. The legal battles resulted, as Frost notes, in establishing precedents and altering the climate of legal opinion, which would help make a repetition of the Mooney case almost impossible today. The shorter-term yield was to bring out the true facts of the case and thereby lay the groundwork for political intervention once the conservative hold on the California governorship was broken. This finally happened with the election of Culbert L. Olson in 1938, and Mooney and Billings forthwith received pardons.

Frost tells this complicated story in exhaustive detail. Having done a remarkably thorough job of research, he unravels the case in all its twists and turns with clarity and precision, and he judiciously indicates what is certain, what is likely, and what remains unknown (including the actual perpetrators of the outrage). Frost excels in delineating the legal intricacies of the Mooney case. Its meaning for labor and the Left, however, gets rather lost in the welter of detail. One wonders, finally, whether the subject really warrants the devotion Frost lavished on

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it, whether a briefer, interpretive monograph might not have been more suitable than nearly five hundred pages of closely printed text that were seventeen years in the making.

University of California, Davis DAVID BRODY

CHARLES FRANCIS MURPHY, 1858–1924: RESPECTABILITY AND RESPONSIBILITY IN TAMMANY POLITICS. By *Nancy Joan Weiss.* [Smith College, The Edwin H. Land Prize Essays.] (Northampton, Mass.: Smith College. 1968. Pp. x, 139. $2.00.)

THE DECLINE OF THE PROGRESSIVE MOVEMENT IN WISCONSIN, 1890–1920. By *Herbert F. Margulies.* (Madison: State Historical Society of Wisconsin. 1968. Pp. ix, 310. $5.95.)

EMBATTLED DEMOCRACY: MISSOURI DEMOCRATIC POLITICS, 1919–1932. By *Franklin D. Mitchell.* [University of Missouri Studies, Volume XLVII.] (Columbia: University of Missouri Press. 1968. Pp. 219. $4.75.)

SOME central questions that puzzle twentieth-century American political historians concern the nature of progressive reform, the basis of its support, the fate of the progressive movement, and the role of cultural factors in influencing political behavior. These three books, one an undergraduate honors thesis and two extended doctoral dissertations, cover these questions.

Nancy Joan Weiss's study of Charles Francis Murphy was originally prepared as an undergraduate honors thesis at Smith College. It is an impressive performance, based on extensive research. Her main thesis is that Murphy was a different kind of a boss than his Tammany predecessors; he turned "the machine toward some new and creative ends." In this she follows the theme of J. Joseph Huthmacher, whose article, "Charles Evans Hughes and Charles Francis Murphy: The Metamorphosis of Progressivism," appeared in *New York History* (XLVI [Jan. 1965]). Huthmacher suggested, as he had in an earlier influential article, that historians have neglected urban immigrant support for progressive measures and overestimated the role played by middle-class reformers. Weiss, not surprisingly, agrees with this conclusion. Her discussion of Murphy's rise and the functions of the Tammany machine is generally sophisticated, but she encounters difficulty in explaining Murphy's motivation in supporting reform legislation and liberal politicians such as Al Smith and Robert Wagner. On one page she notes that "Murphy's willingness to play reformer illustrated his sense of civic responsibility," while on the next she tells us that Murphy's "market-basket liberalism" was merely an extension of "the technique that had always kept machine politics going." It is hard to have it both ways. Either the boss was becoming respectable and opting for reform or he was bending with the wind in the interests of self-preservation.

Actually, as several historians have recently noted, there were other bosses who accommodated progressive innovations and remained in power long after the reformers had departed. Boss support of welfare legislation and limited political reform, however, does not make them "progressives." To maintain that it does, as do both Weiss and Huthmacher, is to overlook Samuel P. Hays's important point that progressive urban reformers sought not only to alter the structure of

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government but also "to change the occupational and class origins of decision-makers." Urban politicians with immigrant working-class constituencies may have sought welfare and regulatory legislation in the years after 1912, but this was essentially a phenomenon with a different social and ethnic base than the earlier reform movement, and it is important to distinguish between the two.

The changing nature of both progressive reform and its base of support is illustrated in Herbert F. Margulies' study. In charting the ebb and flow of Wisconsin progressivism, Margulies notes that many early supporters of Robert M. La Follette were alienated by the New Deal-like measures of the Francis E. McGovern gubernatorial administrations (1910–1914). By 1914 factionalism among Wisconsin progressives, much of it caused by La Follette's difficult personality and his tactics, voter discontent with prolabor and welfare measures, and the progressives' loss of Democratic support, resulted in the election of the Stalwart Republican Emanuel Philipp as governor. The chapters that discuss the impact of the war and the loyalty issue upon Wisconsin politics during Philipp's administrations are especially informative. Margulies shows in detail how the strains of the war years destroyed the old La Follette organization "beyond repair when progressives old and young rallied to the flag." Out of the "wreckage," maintains the author, emerged a new reform coalition that was broader and more radical than that of prewar years and therefore "substantially a new and different phenomenon" than the old progressivism.

Margulies' earlier chapters trace the development and vicissitudes of Wisconsin progressivism, but while they make some use of recent writings such as David P. Thelen's *The Early Life of Robert M. La Follette,* they still do not go very far beyond Robert Maxwell's *La Follette and the Rise of the Progressives in Wisconsin.* Although on occasion Margulies does break out of the narrative form, his analysis of the role of cultural and ethnic factors and urban-rural tension is often incomplete. It is unfortunate that he does not analyze all his election data with the same thoroughness he applies to the 1918 and 1920 contests.

Historians concerned with the impact of ethnocultural factors upon politics will find Franklin D. Mitchell's *Embattled Democracy* the most satisfactory of the three books reviewed here. Although it does not have the range nor encompass the research of Margulies' book on Wisconsin, it clearly explicates the elements shaping Missouri Democratic politics from 1919 to 1932. Mitchell patterns his book on J. Joseph Huthmacher's *Massachusetts People and Politics, 1919–1933,* although he does not match Huthmacher's depth and perception. Mitchell examines the impact of the fight over the League of Nations, farm policy, prohibition, and the Ku Klux Klan on Missouri Democrats and the fortunes of its most prominent figure, Senator James A. Reed. He carefully delimits the role of these factors in splitting the party and fostering urban-rural tension.

Prohibition was probably the key issue in Missouri during the 1920's. As Mitchell notes, it was a "persistently disruptive factor in both parties, since it reversed the traditional partisan response of many Democrats and Republicans." In 1928 the liquor issue helped Al Smith carry traditionally Republican St. Louis while the votes of outstate dry Democrats allowed Hoover to win the state. The cultural issue of prohibition also seemed more potent than economic matters, carrying the wet conservative Bennett Clark to a victory over the dry progressive

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Charles M. Hay in the 1932 Democratic senatorial primary. Another important development in Missouri politics during these years was the movement of normally Republican Negroes into the opposing party, a move prompted by Democratic wooing and Republican neglect of the Negro voter. By the 1932 election, the party had healed the abrasive cultural and economic splits of the past generation and faced the Republicans with a "broadened coalition of farmers, laborers, old-stock Irish and Yankees, Negroes and newer ethnic groups, wets and drys. . . ."

These three books add to our knowledge of the forces shaping American urban and state politics during the first third of this century. We still do not know enough, however, about the ethnocultural base of each party and of the factions within the parties. Neither do we have a clear perception of the basis of the appeal of parties nor the sources from which the parties and their factions drew their votes. In spite of the many books that have been written about progressive period politics, this criticism still applies to that era. Until we have studies that fill the ethnocultural gap, we will fail to understand fully the nature of twentieth-century American politics.

Carnegie-Mellon University JOEL A. TARR

ALFALFA BILL MURRAY. By *Keith L. Bryant, Jr.* (Norman: University of Oklahoma Press. 1968. Pp. xiii, 287. $6.95.)

THE depressed 1930's witnessed the rise of a rash of buffoon-like politicians who became governors of their states and then set out to win the highest political office in the country. One such person was Alfalfa Bill Murray, a native of the frontier village of Toadsuck, Texas, who farmed, taught school, worked as a newspaper reporter, sold books, organized Farmers' Alliances, ran for political office, edited newspapers, and practiced law. After being elected governor of Oklahoma Muray made a ludicrous bid to win the Democratic nomination for President in 1932 on a platform of "Bread, Butter, Bacon and Beans." An agrarian in theory and an opportunist in practice, he in many ways made a mockery out of the high office to which he was elected and a travesty out of democratic practices.

A small-time politician with big-time ambitions, Murray could be counted upon to remember friends and relatives when in high offices. Nepotism and cronyism meant nothing to him. He pretended to fear no one: the oil industry, newspapers, neighboring states, or the President of the United States. His attitudes toward higher education and faculty members were almost as incredible as his personal behavior in and out of office. He removed professors at will, and, in the words of the author, he "attacked university professors who failed to teach eight hours a day, six days a week, twelve months a year." While governor he called out the National Guard no less than forty-seven times for duties ranging from the supervision of the sale of University of Oklahoma football tickets to guarding the oil fields. Tax relief for the small man saved his administration as governor from being a complete failure.

The volume has many commendable features. Bryant is scrupulously fair in his treatment of this stubborn and unpredictable man, and he has written the best biography of Murray in print. The text is highly readable and based on the best available primary sources. It promises to hold its place for some years to come.

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The author's main concern with Murray the politician has unfortunately caused him to lose sight of Murray's main goal to save the family farm. What treatment there is of this is very subordinate and incidental. Why did he and countless others fail to accomplish this Herculean feat? It also appears that topics such as tenant and farm labor problems, droughts and dust storms, especially in a state overrun with them, merited serious attention at a time when it had a governor who claimed he cared for the farmer. These limitations, however, should not detract from a study that deserves to be read by all students of American politics.

University of California, Los Angeles THEODORE SALOUTOS

THE BRAINS TRUST. By *R. G. Tugwell.* (New York: Viking Press. 1968. Pp. xxxii, 538. $10.00.)

THIS large volume deals with Roosevelt's campaigns in 1932, beginning with the formation of the brain trust and ending with victory in November. Those who know the writings of Moley, Fusfeld, Freidel, Sternsher, and Tugwell himself will find the tale familiar, but will learn more about the author. The work focuses upon him and discusses events as they looked to him in so far as his memory, aided by a few notes, can reconstruct them. Once again he reveals his fascination with the history with which he has been involved and the major figures with whom he has been associated and his compulsion to write about them. (He may be influenced by lack of confidence in historians, for he has concluded "that their views are very often views and not actually history.") A tougher editor could have improved the book, chiefly by cutting and pruning and by forcing the author to define his groups more clearly, but it has many virtues, including excellent descriptions of people, their interrelations, their ideas, their moods, their strengths, and their weaknesses. Louis Howe, according to Tugwell, "wouldn't have known a tomato plant from a pole bean," and behind Garner's façade was "a singularly restricted understanding of the events he was caught up in."

Tugwell does not attempt to explain Roosevelt's nomination and victory. His central concerns are, instead, the intellectual in politics and the functioning of advisers. He writes, in a sense, an intellectual history of the campaign. He takes us behind the scenes in the Roosevelt camp and provides especially rich descriptions of his own conversations with the candidate, their efforts to educate one another in economics and politics, and their many disagreements.

For historians of the New Deal, the most significant features of the book are its accounts of Roosevelt's resistance to change and of Tugwell's frustrations. A short time ago, in *FDR: Architect of an Era* (1967), it appeared that the sense of frustration that Tugwell had felt three decades earlier had faded from his memory. Now it is obvious that the pain is still there. "What worried me," he writes of 1932, "was that he [Roosevelt] was being much more considerate of those who were afraid of change than of those who wanted it." "As the active campaigning came on, Roosevelt seemed to me more and more concerned with pleasing the 'practical' people." "I had substantially failed," he concludes, "and I regretted that I had not been more effective."

Tugwell had failed to give to Roosevelt's campaign the orientation he thought the situation demanded. He wanted the candidate to emphasize recovery, propose

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deficit spending as the means of achieving it, and advocate collectivism involving coordination, management, and planning. Roosevelt, focusing his attention upon victory, rejected this advice, insisting that political realities demanded rejection, and emphasized the ideas that came to him from the elder statesmen in his party, from Howe, from spokesmen for business leadership such as Bernard Baruch, and from the Brandeisians. Even the leader of the brain trust, Raymond Moley, urged Roosevelt to proceed more cautiously than Tugwell advised. Baruch stressed the restoration of business confidence by cutting back on government spending and balancing the budget, while Brandeis advocated the restoration of competition. "Baruch and Brandeis, taken together—although each detested the other—had smothered our arguments."

Despite his frustrations, Tugwell did not withdraw and sulk. Instead, he remained close to Roosevelt for nearly five years. He did not allow troublesome comments by other intellectuals to shatter his self-confidence nor generate doubts that he remained an intellectual. He could argue, have a say, and try to exert influence. He had a significant opportunity and tried to make the most of it. He continued to hope that the politician would break away from Baruch, Brandeis, and the others, if not during the campaign then later. But, in a note at the end of the book Tugwell indicates that his relations with Roosevelt during 1932 were much like their relations in subsequent years: "I was always urging something he would not or could not do, and he was always discussing it with interested attention. Sometimes a little of it came through, but not much." From Tugwell's vantage point, in other words, America after the New Deal remained too close to what it had been before.

Two members of the brain trust, Tugwell and Moley, have now supplied valuable accounts of their experiences. It is to be hoped that the third member, Adolf A. Berle, Jr., will soon produce the major memoir that he is capable of writing.

University of Missouri RICHARD S. KIRKENDALL

NEVER AGAIN: A PRESIDENT RUNS FOR A THIRD TERM. By *Herbert S. Parmet and Marie B. Hecht.* (New York: Macmillan Company. 1968. Pp. xii, 306. $6.95.)

HERBERT S. Parmet and Marie B. Hecht, who previously collaborated on a biography of Aaron Burr, have turned their attention in this book to the presidential election of 1940. They have prefaced their description of the election with a brief account of the third-term tradition, and they conclude their study with a short chapter devoted to the Twenty-second Amendment. The authors have based their book on a variety of manuscript collections and published sources, but the bibliography for their undocumented narrative gives no indication that they consulted the Frank Walker Papers and the doctoral dissertations of Hugh Ross and Henry O. Evjen (two articles published by these two men are noted, however).

The authors, for the most part, cover very familiar ground, and they fail to provide any new information on such matters as exactly when President Roosevelt decided to run for a third term, whether Harry Hopkins was representing the President at the Democratic National Convention, and the allegations concerning the packing of the galleries with Willkie supporters at the Republican National

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Convention. They successfully counteract the "oversimplification" that Willkie's candidacy was "engineered and dominated by Wall Street," but this is hardly a novel conclusion. They devote some attention to Nazi efforts to influence the outcome of the election, but they have nothing to say about how *The German White Paper* came to be published in the United States, and they ignore the publication of the so-called Potocki report just before the election. The authors conclude that the attitude of the voters toward intervention in the European war was "the greatest single reason" why the 1940 vote deviated from the 1936 electoral pattern and that Roosevelt himself was probably the principal cause for the final result.

Although Parmet and Hecht devote a few pages to an analysis of the 1940 vote, their approach to their subject is primarily descriptive rather than analytical, and only rarely do they probe beneath the surface of events. They make only superficial comments about the structure and character of the two parties, and they are insufficiently concerned with the politics of the New Deal. Their book may prove to be of some interest to the general reader, but it offers very little to the specialist.

University of Michigan SIDNEY FINE

THE RIGHT OF REVOLUTION. By *Truman Nelson.* (Boston: Beacon Press. 1968. Pp. 148. $4.95.)

TRUMAN Nelson's brief book contains three main elements that are loosely interwoven: a sustained and impassioned plea for support of revolt by black people in the United States today; a sympathetic description of the "black uprisings" of the summer of 1967 and of two black revolutionists of the 1960's, William Epton and Robert Williams; a justification of revolt by blacks and their allies through appeals to the concept of the "right of revolution" in the history of the United States.

Nelson's own words convey better than any descriptive phrases the urgency of tone, the style of intellectual analysis, and the ideological positions that characterize the volume. "I believe," he writes, "that the eighty riots in the ghettos of America in the summer of 1967 were revolutionary alternatives to present degradation and future genocide. . . . If I deny black people . . . any . . . form of resistance for which white men have been applauded . . . I am not only a coward, but a racist. . . . I must join them, defend them, explain them . . . recruit for them. Their history alone has become our only redemption, the only road to American fulfillment, . . . to a life of principle and ideals, to the answers to the great and accursed questions of personal independence, the citizen's relation to the state, the inalienable right of resistance, the wrongs of war, poverty, racism, and every other form of human denial. I do not wish, any longer, to be guilty concerning my brother."

The primary historical component of the book is its appeal to the tradition of the "right of revolution" in the United States as warrant for revolt today. Nelson, like other revolutionists, holds that "history is on our side," and those words provide the title for one of his chapters. The history he cites comprises either words or actions in support of the "right of revolution" by such Americans of the eighteenth and nineteenth centuries as Jefferson, John Adams, J. Q. Adams, Thoreau, John Brown, Theodore Parker, and Lincoln. He does not cite Jefferson's prosecution for treason of Aaron Burr, who allegedly claimed the "right of revolution";

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nor does he cite the claim of the "right of revolution" as justification for secession by some white southerners or Lincoln's opposition by force to that claim. Thus, Nelson's history, to me, omits the complexities of the subject.

It seems apparent, however, that Nelson's purpose was not to write a book about historical complexities but rather to issue, as he has done, a ringing call to stop weighing alternatives, to cease temporizing, and to undertake revolution. D. W. Brogan in *The Price of Revolution* (1951) suggested that "you can have too much of a good thing, even of that strong remedy for the ills of a body politic, revolution." Nelson's message is the direct opposite of Brogan's. Will Brogan's *Price of Revolution* or Nelson's *Right of Revolution* seem the more significant and the more illuminating volume a generation from now?

University of Washington THOMAS J. PRESSLY

SAM JOHNSON'S BOY: A CLOSE-UP OF THE PRESIDENT FROM TEXAS. By *Alfred Steinberg.* (New York: Macmillan Company. 1968. Pp. 871. $9.95.)

MANY recent books have analyzed the institution of the presidency or described the experiences of a given President. Generally the authors have emphasized the difficulties the President had with Congress, the bureaucracy, and the public. Directly or indirectly these writers pointed to the need for increased power for the President. But in 1966, Senator J. W. Fulbright published *The Arrogance of Power;* the following year Senator Eugene McCarthy published *The Limits of Power,* and Theodore Draper published *Abuse of Power.* Among other things these authors expressed deep concern over the way the power of the presidency and the power of the nation were misused by President Lyndon B. Johnson.

Alfred Steinberg, who has written studies of Eleanor Roosevelt and Harry S. Truman, in his current book pursues in relentless detail Johnson's unceasing quest for personal power and wealth. After describing Johnson's acquisition of a rich television chain through influence with the Federal Communications Commission in the early 1950's, Steinberg writes: "So the Johnson money power-drive nurtured by his aggressive political power advanced rapidly and easily. And his political power set upon the security of several million dollars moved forward with similar ease."

With a real sensitivity for the inner maneuverings of aggressive politics, Steinberg traces Johnson's political career from the influence of his father, a Texas state representative, to his decision on March 31, 1968, not to run for re-election. Those who are puzzled as to how Johnson dissipated his huge 1964 victory and decided on March 31, 1968, not to run again will find Steinberg's lengthy study useful. Whether it is a description of Johnson as a congressional secretary to Representative Kleberg, Johnson's election to the House of Representatives in 1937, his defeat for the Senate by "Pappy" O'Daniel in 1941, the 1948 senatorial "landslide" victory by the margin of eighty-seven votes as a result of "corrected totals," his role as Senate majority leader, or in the White House, Steinberg emphasizes Lyndon B. Johnson's brashness and talkativeness, his scheming and ruthlessness. Steinberg concludes: "He was only Sam Johnson's boy, who had grown up poor and earthy and ambitious in the Hill Country, and spent his lifetime fighting

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for personal power and wealth. In his unquenchable thirst for both, he failed to acquire any other purpose to guide his decisions, though he developed a remarkable talent for judging those who stood in his path. He learned how to seize authority from the lazy or slow, threaten and storm at the weak, flatter the vain, promise the greedy, buy off the stubborn, and isolate the strong. . . ."

It is unfortunate that this book lacks documentation. Steinberg is widely read in the secondary literature (except that he fails to mention the *Texas Observer,* the liberal Austin weekly that has long been a critic of Johnson). He also interviewed many people to piece his story together. He explains that a number of those he interviewed permitted him to examine letters, reports, and other personal material. But the value of all this effort is limited for the historian since there is no attribution. Some undoubtedly insisted on no attribution, but those who did not should have been cited.

At times the book contains emotion-laden language or needless statements, as, for example, "dripped with unctuous praise," "worn-out has-been," "Lady Bird, who still sat in theater balconies when she went alone and informally to plays," "they drank themselves into savage animals." Moreover, Steinberg accepts Truman's version that he was responsible for Adlai E. Stevenson being nominated on the third ballot in 1952. Actually, had Steinberg consulted *Presidential Nominating Politics in 1952: The National Story,* by Paul T. David, Malcolm Moos, and Ralph M. Goldman (1954), he would have been on more accurate ground. Averell Harriman drafted a statement asking his supporters to vote for Stevenson on the third ballot before Truman sent Harriman a message asking that he withdraw.

It will be difficult for historians to put the Johnson jigsaw puzzle together. Johnson, like Truman, will probably rewrite much of history. Books like Steinberg's *Sam Johnson's Boy: A Close-Up of the President from Texas,* and Robert Sherrill's *The Accidental President* (1967), however, can be useful as antidotes. They both reflect a deep concern over the fundamental question of power for what end. Errors of fact in the Steinberg book may well be unearthed by later research among unpublished papers, but Johnson's pursuit of power and his use of power as analyzed by a contemporary observer should still prove valuable to the historian.

University of Hawaii WALTER JOHNSON

SAFEGUARDING CANADA, 1763–1871. By *J. Mackay Hitsman.* ([Toronto:] University of Toronto Press. 1968. Pp. xii, 240. $6.50.)

THIS book, we are told, was written to meet the need for studying the development of Canada's defense policy within a military rather than an almost exclusively political or constitutional context. While it is arguable that this approach has not been neglected by students of the Dominion's military affairs, Mr. Hitsman has effectively reconstructed the varied and complex problems that confronted the strategists at Whitehall when they settled down to the business of planning for the defense of Canada. These problems ranged from geographical hazards and diplomatic difficulties, through the attitudes of British politicians and the reaction or

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response of the colonists themselves, to the heavy demands imposed on the Empire's military establishment.

Even though the author recognizes the necessity of placing the theme in a larger imperial framework, he does leave the impression that safeguarding Canada was the only chore that really mattered at either the War Office or the Colonial Office. (As Lord Clarendon gloomily remarked in 1854, Britain had something like forty colonies to look after all over the world, this at a time when the mother country was about to go to war with a major power in Europe.) As with so many Canadian histories, this one is in places uncomfortably self-absorbed, as though the whole of the imperial world spent nearly every waking moment pondering the security of the "true north strong and free."

A more serious criticism is that the latter part of the book seems to be little more than a condensation and rephrasing of a work—*Canada and the British Army, 1846–1871*—published some thirty years ago by C. P. Stacey, even, indeed, down to a chapter headed "Federation and Fenianism." Admittedly the only appropriate place to end, given the topic, is 1871, the year that saw the withdrawal of the imperial legions from the young Dominion.

On the credit side, Hitsman has presented a lucid and sometimes illuminating work, especially when he deals with the period 1814–1850 and with the steps taken to enhance the security of British North America during the Canadian rebellions and recurring boundary squabbles with the United States. The book adds another dimension to our understanding of pre-Confederation Canada.

McMaster University C. M. Johnston

LAND POLICIES OF UPPER CANADA. By *Lillian F. Gates*. [Canadian Studies in History and Government, Number 9.] ([Toronto:] University of Toronto Press. 1968. Pp. 378. $7.50.)

Edward Gibbon, working on land rents in the later Roman Empire, remarked that it was "un travail sec et ingrat, mais quand on construit un édifice, il faut en creuser les fondements." The history of land use or of land policy has always been a tangled, even an intimidating business; the patience and persistence with which Mrs. Gates sorts out its history in Upper Canada is admirable. In these days of instant scholarship, there is much to be said for the solid, detailed book—this one twenty years in the making—that has no pretenses, none of that "agreeable, superficial, and inaccurate performance," to quote Gibbon on Voltaire.

The initial policy of the British government in Upper Canada was a liberal rewarding of the military and the loyalists according to rank and station. A generous reserve was set apart for the Established Church, and also for the crown. The land of Upper Canada was regarded by British administrators as a supply of capital, nearly inexhaustible, that might be used to endow men and institutions to buttress established government. That was a great misapprehension. The only real capital was the labor the settler put into the land and the source of much of its contemporary value. It took time for these elementary facts to be appreciated; meanwhile, the aristocratic principles crossed the purposes of many of the settlers. The chronic grievance in a pioneer community is privilege, and there were many forms of it. Thus the legislature was often called in to attempt to redress acts

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of executive government, either bad policies or sometimes bad administration of policies in themselves not unreasonable.

It is striking how difficult it was, even when there was agreement between executive and legislature, to legislate effectively against abuses. Numerous expedients were tried: human beings were just too clever. The wild land tax, for example, was designed to prevent, or at least weaken, absentee ownership of unimproved land. When these lands became delinquent for taxes, they were sold, but they were bought up by a few men with an eye for a good investment who had eight years to speculate with them before the lands had again to be forfeited.

The truth was that land was the only way of making a fast dollar, and speculation in it, by every device, was inevitable. Consequently, it was nearly impossible to get lands adequately administered; the administrators themselves were often trafficking in them. The problem was how to give a man who wanted to settle and work the fairest chance of doing so. It is not as easy as it sounds. This book unravels, over a long and difficult history, just why.

Dalhousie University P. B. Waite

A SEARCH FOR STABILITY: UNITED STATES DIPLOMACY TOWARD NICARAGUA, 1925–1933. By *William Kamman*. [International Studies of the Committee on International Relations, University of Notre Dame.] (Notre Dame, Ind.: University of Notre Dame Press. 1968. Pp. xiii, 263. $7.95.)

Few topics in American history and diplomacy have been accorded more attention by writers in either the United States or Latin America than have the relations of the United States with Nicaragua since the middle of the nineteenth century, and especially since the year 1900. Professor Kamman's monograph is, nevertheless, by no means superfluous, for it deals more thoroughly and impartially with an important eight-year relationship of the United States with Nicaragua than do many preceding volumes, nearly all of which have discussed this period and are familiar to Kamman, as is indicated by his long bibliographical essay.

Except for an occasional mixed metaphor, the author's literary style is outstanding for precise diction and avoidance of complicated sentences. Only one of his twelve chapters seems defective in any important respect: Chapter 1 undertakes too much by way of introduction in attempting to cover the period since 1502 within the space of seventeen pages. In my opinion, a summary of relations between the United States and Nicaragua during the years 1893–1924 would have been sufficient. Moreover, the title of this first chapter—"Nicaragua and the United States by 1925"—seems a little queer, as does the subtitle of the entire volume, especially the phrase *Diplomacy toward Nicaragua*. Although only eight years are included, the story is very complicated, and it involves many political and military leaders who probably should have been more fully identified, perhaps in an appendix.

But these are very minor defects in what, for the most part, is an excellent monograph dealing with an important period in inter-American relations, an era of transition culminating in the Good Neighbor policy.

University of Chicago J. Fred Rippy

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EDUARDO SANTOS AND THE GOOD NEIGHBOR, 1938–1942. By *David Bushnell.* [Latin American Monographs, Second Series, Number 4.] (Gainesville: University of Florida Press. 1967. Pp. 128. $3.75.)

COLOMBIA, more than most Latin American nations, had good reason to dislike, distrust, and fear the United States during the early decades of this century. Added to basic religious-cultural differences, which often erupted in persecution of Protestant missionaries, were specific grievances about the United Fruit Company's banana monopoly, the illegal activities of American oil companies, and the defaulted dollar bonds of the 1920's. Above all, there was bitterness over the United States-sponsored Panama Revolution of 1903 and the consequent loss of the isthmus. As the conservative newspaper *El Siglo* put it, in attacking the mutual defense agreements with the United States in 1940, Colombia was coming to the aid of "the very power that mutilated our territory."

Professor Bushnell's able monograph, based on Colombian newspapers and unpublished diplomatic correspondence in the US National Archives, tells the story of the Roosevelt administration's successful efforts to end old controversies and establish a basis for wartime cooperation. A *rapprochement* that began in 1922, with the payment of a $25,000,000.00 indemnity for Panama, became closer during the early 1930's, and reached a culmination during the administration of Liberal President Eduardo Santos (1938–1942). Colombia, the first nation to break relations with Germany, agreed to sell its entire output of critical materials like platinum and rubber to the United States, eliminated German-born pilots and administrators of the national airline, and permitted American ships and planes to enter its territorial waters in pursuit of submarines.

The success of the Good Neighbor policy, as Bushnell shows, owed much to economic considerations. As European markets and supplies were cut off by the war, trade with the United States became overwhelmingly important to the Colombian economy; exports to the US rose from 58 per cent of the total in 1938 to 92 per cent in 1942, including 99 per cent of Colombia's major export, coffee. A striking example of how economic power could be translated into political results was the taming of the anti-US *El Siglo*. That paper's anti-Americanism and praise of Adolf Hitler's "gigantic" work were brought to the attention of the State Department by Ambassador Spruille Braden, with the request that the information be passed along to companies doing business in Colombia. Shortly afterward, advertisements for products like Camel cigarettes and Vicks VapoRub began to be withdrawn, and the New York supplier of scarce newsprint made pointed inquiries about the paper's pro-Nazi tone. *El Siglo*'s owner hastily arranged an interview with the ambassador, promised to reform, and kept his promise; soon the paper was editorializing that "outside of inter-American solidarity, there is no salvation." Amicable relations between the United States and Colombia, based upon economic realities and Good Neighbor diplomacy and aided by occasional judicious arm twisting, have endured to the present day.

University of Notre Dame SAMUEL SHAPIRO

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SANTA ANNA. By *Oakah L. Jones, Jr.* [Twayne's Rulers and Statesmen of the World Series, Number 6.] (New York: Twayne Publishers. 1968. Pp. 211. $4.95.)

No student of Mexican history can ignore Antonio López de Santa Anna. Whether Santa Anna deserves a place of pre-eminence, as the author of this book maintains, is questionable, but at least he deserves an explanation. Major Jones provides none.

A stated purpose of this book is to attempt "a concise general survey of Santa Anna's life." The author does this unimaginatively by tracing the general's political and military career, largely from 1821 to 1855. Another aim was to use new sources available over the last three decades. The magnificent British diplomatic and consular dispatches from Mexico, available in the Public Record Office and on microfilm in the Bancroft Library, have, however, been ignored, and US dispatches almost ignored. The documentation is overwhelmingly secondary and often badly handled, as, for example, using Callcott's citation of Ward's classic *Mexico in 1827* when Ward's work is readily available. Nor were the Mexican archives touched, in spite of the fact that much new material has been made available to scholars. The author also attempted "to depict Santa Anna in relation to the period in which he lived." To have achieved this last goal would have taxed the skills of a master historian and required a major contribution to the social and economic history of early national Mexico. This volume fails to relate Santa Anna to much of anything.

The general reader might derive some benefit from the availability of the book, but since W. H. Callcott's *Santa Anna* was reprinted in 1964, even this is doubtful. Santa Anna has been an enigma, and we have needed a definitive biography. That situation remains unchanged.

Monterey Peninsula College N. Ray Gilmore

TRINIDAD IN TRANSITION: THE YEARS AFTER SLAVERY. By *Donald Wood*. (New York: Oxford University Press for the Institute of Race Relations, London. 1968. Pp. x, 318. $6.25.)

This distinguished monograph was sponsored by Britain's Institute of Race Relations, and it is a model study of its type. Its author is presently lecturer in history and assistant dean of the School of African and Asian Studies in the University of Sussex. His meticulous survey covers the critical era in this major Caribbean island's development between the close of apprenticeship in 1838 and the enlightened, realistic administration of Sir Arthur Gordon between 1866 and 1870, by which time a new society was evolving. It is the product of extended research, meets the highest canons of historical scholarship, and is eminently readable.

Trinidad occupied a unique position among Britain's Caribbean possessions. This former Spanish colony was not added to the Empire until 1802. Numerous French Creoles had meanwhile found refuge from the Revolution there. Its peoples were Catholic, and the conquerors were Protestant; their ways of life differed profoundly. Because of the island's overwhelming non-British population, representative government could never be instituted, and, with financial control in the governor's hands, the planter-merchant class never gained political power.

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An abundance of land permitted ambitious former slaves to become independent producers, notably of cacao, a hitherto neglected crop. The large-scale sugar producers, confronted by an uncertain local labor force, undertook to attract newcomers by organizing immigration societies operating through London. Workers were drawn from adjacent colonies, the United States, Madeira, Sierra Leone, and Saint Helena, havens for unfortunate Africans snatched from the hands of illicit slave traders. No less than 3,198 came from the last area alone by 1861. Most newcomers became estate hands, but shortages were chronic. Small numbers of Chinese coolies did little to meet the situation. A sound solution at length appeared in the carefully regulated entry of indentured Indians, starting in 1845. Many of these remained after their stipulated services had terminated.

A multiracial society thus emerged. Earlier conflicts between Spanish, French, and English residents and between whites and blacks were now accentuated by new tensions involving dusky Indians and yellow Chinese. A grimly determined effort by a powerful British plutocracy to Anglicize the community had ominous potentialities that were averted, fortunately, through its leader's downfall in financial scandals. This welter of hatreds, rivalries, and misunderstandings was ultimately resolved along sensible lines set out by Gordon, and a distinctive society, cemented by a rich, blended Trinidadian culture, has resulted. This is a fascinating account indeed!

Alaska Methodist University LOWELL RAGATZ

BRITAIN AND THE ONSET OF MODERNIZATION IN BRAZIL, 1850–1914. By *Richard Graham*. [Cambridge Latin American Studies, Number 4.] (New York: Cambridge University Press. 1968. Pp. xv, 384. $9.50.)

PROFESSOR Graham has written a good account of the British contribution to the modernization of Brazil during the period 1850–1914. Developments in the economic sector—railways, communications, export-import trade, banking, manufacturing industries, engineering, technology, entrepreneurship—furnish the subject matter for about half of the book, while "changes in values, ideas, and world-views" also receive attention. There are chapters on the abolition of slavery, the rise of the urban way of life, the influence of Herbert Spencer and the idea of progress, the religious factor, the problems of freedom and association, and "Middle-Class Britain and the Brazilian Liberals."

Graham is cautious and objective in his attribution of credit or blame to the British in specific instances of modernization. For example, leading British financial and trading interests were hostile to industrialization in Brazil and in some cases resorted to drastic action to throttle potential competition. The bigger the British exporter, the smaller was his contribution to Brazilian modernization. Furthermore, "most of the foreign loans did little to advance modernization." The pros and cons as regards the British contribution are ably summarized in the conclusion.

Caution is also displayed in the treatment of the problem of causality, but in the conclusion the statement is made that the onset of modernization in Brazil "was not caused" by the British, but rather "was the result of broad trends within which Britishers exercised an important but limited role." Earlier in the book the

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author had stated that the reasons why Brazil, despite its early connections with the modern world, had not entered the mainstream of development "lies outside the scope of this work."

Graham's theme is modernization, but he is impressed by the extent to which traditional elements still survive in Brazil. "By 1914," he points out in the preface, "Brazil had done no more than begin to modernize"; he doubts whether "even now" Brazil has entered that stage, typical of modern societies, where there is a "steady stream of technological innovations."

There is no attempt in the book to define "modernization" nor to set forth a theory of social change. In the preface the author also states that "it is not my purpose to add to the extant literature on the theory of modernization." This is understandable, but there are times when the reader feels that the significance of the study would have been enhanced had he raised some questions as to the adequacy of the traditional-modern dichotomy to uncover the real roots of some of the issues. At the same time, he provides many insights that will be useful to other workers in the field. Since this volume is confined to British modernizing influences, with only passing reference to other nationalities, additional studies are needed to provide a vantage point from which to survey the whole field.

Washington, D. C. GEORGE WYTHE

DIEGO BARROS ARANA. By *Ricardo Donoso.* [Instituto Panamericano de Geografía e Historia, Publication Number 226. Comisión de Historia, Number 88. Historiadores de América, Number 12.] ([México, D. F.:] the Instituto. 1967. Pp. xvi, 414.)

WITH interest in the history of Latin America increasing, this volume is timely. It not only provides insight into the life and times of a great nineteenth-century Chilean historian, but it is also, in itself, a worthy example of twentieth-century Chilean historiography. This volume is a reprint of Donoso's earlier *Barros Arana, educator, historiador, y hombre público* (1931). The author tries to give the reader a better understanding of the importance of Chile's greatest historian, not just as a historian but also as a man of letters, a public servant, and an educator.

Diego Barros Arana, born into a prominent politically oriented family of Santiago in 1830, began his literary career in 1850 and thereafter wrote prolifically until his death in 1907. In 1855 he was elected to the faculty of the University of Chile and twice served as rector of that institution. He also led an active political life and, because of his attacks on the Montt administration, was forced to go into exile. His exile was spent in research and travel. Later he was to serve in various diplomatic capacities relating to Chile's boundary disputes.

Barros Arana's most important contribution, and his most outstanding work, was his *Historia jeneral de Chile,* to which several generations of historians are indebted. His other writings were numerous and covered topics from Tupac Amaru to independence, the early national period, and subjects of contemporary interest. He was a competent researcher and could arouse immediate interest in his work.

The value of Donoso's work is not merely that it provides a detailed account of the life and work of Chile's great historian; it also provides a picture of nine-

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teenth-century literary and cultural developments. The work contains 282 pages of text and various appended documents relating to Barros Arana's career, especially his role as an expert adviser on the question of Chile's boundaries. Of particular importance is the Barros Arana bibliography. His works are listed chronologically beginning in 1850 and continuing through 1907. There is also an ample bibliography listing the sources used, and the work is well footnoted. In short, the work is all-inclusive and deserves the attention of students of Chilean history.

Appalachian State University CARL A. ROSS

MEXICAN MILITARISM: THE POLITICAL RISE AND FALL OF THE REVOLUTIONARY ARMY, 1910–1940. By *Edwin Lieuwen*. ([Albuquerque:] University of New Mexico Press. 1968. Pp. xiii, 194. $6.95.)

PROFESSIONAL officers of the Federal Army supported the Díaz civilian government and became a symbol of the despotic regime. The Carrancista Constitutionalist Army of 1913–1917 destroyed this old-style militarism by August 1914. With almost every vice of its predecessor, revolutionary militarism dominated national politics for the next twenty-five years. The present work explains how the army was reformed, professionalized, and brought under effective civilian control by 1940. Civilian-minded Presidents, from Carranza to Cárdenas, and their War Ministers pushed reorganization as rapidly as circumstances permitted. Most revolts for two decades were attempts of politically oriented revolutionary generals to preserve their dominance over civilians. Obregón, Serrano, Amaro, Calles, and Cárdenas, as War Ministers, professionalized the lower ranks of officers first and then reformed recruitment and treatment of enlisted men. Generals, many of them leaders of autonomous regional armies, were bribed, retired, or killed. The army was cut to fifty thousand officers and men, and the military portion of the national budget was reduced from a high of 72 per cent (1917) to 19 per cent (1938), and 7 per cent (1956–1966). Meanwhile, the national government gained varying support from labor, agrarians, and business. Under Cárdenas this became an effective counterweight in the last contest with the generals. These former revolutionaries, now mostly rich, conservative, and "anti-communist," were alienated by Cárdenas radicalism. They continued vocal opposition to the middle-class moderate businessmen and professional men who assumed power after 1940. In spite of their earlier services to the nation, the generals were obsolete, and the last of them were retired in the later 1940's.

The new army is composed of a predominantly middle-class, moderate, professional officer corps, volunteers, and draftees who serve for from one to three years. They are conscious of steadily improved training, provisions, promotional policies, weapons, housing, and the like. Added to earlier education, which makes them literate, troops are given training and experience in technical skills needed in civilian life. They are reminded that they are civilians, with voting rights and responsibilities to the nation. Successful by 1940, this new system has almost become a national tradition by 1968. The army supports the civilian government and appears incapable of threatening it even if it so desired.

Since his pioneering *Arms and Politics in Latin America* (1960), Lieuwen and

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his students have completed at least six doctoral dissertations showing the changing role of the military. Ultimately "a comparative historical analysis will be attempted." Like all the efforts that have preceded it, the present work is based on archival, special collection, and printed materials that reveal mastery of the task undertaken. The anticipated analysis should prove a most valuable contribution to a field once so poorly understood.

San Diego City College EUGENE K. CHAMBERLIN

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* * * * Association Notes * * * *

The New York Meeting, 1968*

Someone has said or has written that history teaches us nothing about the present; instead, the present teaches us what to look for in the past. While that aphorism seems to provide less help for historians than bourbon in actually getting through the agony of meeting together in one city in one hotel for three days, it does seem to have a certain validity, at least as regards the program for the Annual Meeting in New York, December 28–30, 1968.

Perhaps the most striking example in the program that the present shapes the questions we ask of the past was the twelfth-hour decision to move the meeting from Chicago to New York. Its immediate consequence, aside from letters, telephone calls, and telegrams of outrage as well as praise, was the plenary session held during the evening of December 28, on "Professional Organizations and Political Issues." I do not doubt that, as a further consequence, there will shortly be several historical examinations of American professional organizations and their "failure" to face current political issues.

I can see the paper titles now: "The New Monasticism: The Historical Profession and Social Issues (1960–1969)"; "Clerics in Mufti: The Political Self-Emasculation of the Professional Scholar in America (1936–1967)"; and "Termites in the Halls of Ivy; Presentism and Politics in the New Left Historians." It will be recorded that, at least in 1968, a tentative attempt was made to face the issue raised by President Charles Francis Adams when he addressed the Association in 1901. And, for that record, let it be said that there were at least 1,437 reasons to explain (if not to justify) moving the meeting, and no historian will ever discover them all to the satisfaction of any other historian, or, at least, not to my satisfaction.

In the absence of a history of our Annual Meetings (there will probably be one some day since narcissism is our prevailing vice), one can safely say that the response to a contemporary problem represented by the move to New York, which is now called "Mayor Daley's revenge," was more symbolic of this meeting than it might have been of any recent meeting. Should you look in the program for our other contemporary problems, you will find historians assiduously disagreeing about their various historical backgrounds and using the rhetoric of the 1960's to interpret them to us.

I doubt, for example, that there has been an Annual Meeting in the past ten years where at least one aspect of Negro history has not been discussed. But, in 1968, Negro history has become "Black" history, and it abounds. W. E. B. Du Bois had a session devoted exclusively to him as a sociologist, a historian, and as a

* The AHA *Annual Report*, Volume I, *Proceedings*, will have a complete list of sessions.

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"Negro" (how did that slip in?) nationalist. With that for openers, there were at least eleven other sessions that dealt with race in America and in the world. The only subject more popular was the scarcity of "Black" historians possibly open to job offers.

That more and more historians, prodded by more and more students and by their own consciences, are concerned with the unsatisfactory state of history teaching at all levels was also reflected in the program. It was our hope that at this meeting we would get away from the idea of one or two sessions thrown as a bone to teachers and usually dealing with minor matters of pedagogy, as though those who teach history represent a special caste interested in technique alone. Six sessions dealt with the problem, from the broad sweeping questions raised by Charles Sellers: "Is History on the Way Out of the Schools—and Do Historians Care," to "The Process of Learning in History," to "Teaching Black History," to "World History."

Other present-day problems also reflected in the program included radical history, the history of classes, Vietnam, Cuba, contemporary politics, family history, peace and disarmament, and dissent. Perhaps because President Fairbank is a student of the Far East, non-Western topics were very prominent throughout the three-day period. This might also be because the Association recognizes that the history of India, Africa, China, and the Middle East is as important as the microscopic details of American and European history customarily covered at these Annual Meetings. It was for the latter reason that we deliberately sought non-Western papers and sessions.

Another sign of the times was that, not only were graduate students there looking for jobs and attending sessions; they also, apparently, had their own booth at which various kinds of "radical" historical literature were on sale. I think it is significant (although I am not sure of what) that by the afternoon of the second day all the literature was gone.

The tone of this report, and what it says, may give the impression that the committee set out deliberately to make the program for the Annual Meeting guilty of presentism. This is far from the case. If it happened at all, it happened as much by accident as by design. There was too little time (roughly January through the end of April) to do much more than react to proposed papers and sessions. Certainly there was not enough time to hew out a new policy and stick to it. We were like brokers dealing with various established subdisciplines, each of which had to have representation. But we were concerned that papers and sessions, many of them at least, should deal with some of the broader issues that trouble historians today rather than being devoted exclusively to monographic examination of the past. If we succeeded and it was good, we would like to have the praise for it. If we succeeded and it was bad, or if we failed and it was bad, we must also accept the blame.

Whatever your judgment, the committee was pleased that the Association attempted this past year to cope with the fact that the sociology and the intellectual content of the Annual Meetings are too important to be left in the hands of an uninstructed *ad hoc* committee each year. Under the old system, by which our committee operated, there was not time to give serious thought, and then imple-

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ment that thought, about the purpose and goals of an Annual Meeting for fifteen thousand historians: the program must be assembled, and it must get to the printer. The chairman inevitably became cynical and efficient rather than remaining intellectually involved and inefficient. Under the new system, it is hoped, the umbrella committee, with its longer tenure, will continually re-examine the purposes of the meeting and assist the annual committee in more nearly achieving those purposes.

Newberry Library LAWRENCE W. TOWNER

The Year's Business, 1968

REPORT OF THE EXECUTIVE SECRETARY FOR 1968

In 1968 the Association has had both a busy and a good year. Early in September the Council's last-minute decision to shift the Annual Meeting from Chicago to New York somewhat increased the pressure of business. The reasons considered in reaching the decision were restricted because of the time required to plan a successful meeting; in sequel the Association's affairs have incurred relatively little of the inability to communicate that has been so widespread throughout this political year.

In appraising its position at a time of change from a Democratic to a Republican administration, the Association has reason to feel optimistic about relations with government agencies and organizations. While the Endowment on the Humanities weathered some disappointment concerning its larger prospects, it now seems better able to serve the varied interests of historical studies, particularly those that can be shown to have current meaning as well as intrinsic value. The endowment's one grant to the Association this year was to renew the project of editing select cases from colonial court records that illustrate freedom under law. The Office of Education has been developing procedures and standards under which learned societies like the Association can cooperate more meaningfully than before. The most concrete evidence of this is an increased reliance on a consortium of societies, which is known as CONPASS and which, since July, has been housed by the Association. Long-nourished plans of our Committee on Teaching are embodied in a grant now being made by the Office of Education for an Association project to draw scholarly attention to history education in the schools. This grant is to be administered by Indiana University.

Popular interest in history is as challenging and strong as ever. The Association therefore values the cooperation it has been receiving from Teaching Film Custodians of New York City on a project to produce college-level study materials incorporating cartridges of excerpts from major feature films on various historical subjects. Negotiations with the owners of the films desired has taken more time than was expected, but the Association will exert every effort to advance this project, particularly in view of efforts already generously expended by those individual historians involved.

In another effort this year—to strengthen the position of the National Archives for the sake of historians generally—the Association enjoyed the cooperation of

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the Organization of American Historians and of the Society of American Archivists. No concerted pressure for specific administrative improvement proved possible when the Joint Committee of the three societies reported this summer. Public discussion of the committee's report, and possibly a bill in Congress, may, however, significantly increase public understanding of the important functions of the National Archives.

The Association itself is entering upon a period of consolidation. The remarkable growth in membership since 1953 may be slowing down, but there is not sufficient evidence to establish this. Under the capable management of W. Douglas Harris, our new Business Manager, the handling of our membership files has become more efficient; it has been extended to provide the locator index, within the limits of preregistration, for this meeting.

The *American Historical Review* is now in the scholarly hands of a Managing Editor able to devote four days per week to AHA duties, and prospects for some significant improvements in procedures are in evidence. The Assistant Editors have prepared a handsome index volume to the *Review* for the years 1955–1965. Editorial responsibility for the Service Center pamphlets has been transferred to Mr. Webb from Mr. Zangrando, who this year added several notable new titles to the series, including one by Mr. Fairbank. The new edition of *Preparation of Secondary-School History Teachers,* by Donald B. Cole and Thomas Pressly, is earning particularly favorable comment for the Service Center.

A major event in Association planning this year has resulted from a memorandum that surveys the functions of the Association; it was prepared by the Executive Committee and is now available to interested members. It directs attention to possibilities for better communication and coordination among the many societies whose specialized interests are within the broad field of history. Similar possibilities with other disciplines in the humanities and social sciences have developed promisingly this year within the two contexts of the American Council of Learned Societies and the National Research Council. In the latter case, a focus of attention is needed for a new handling of bibliographies for historical work. Combining close coordination with other societies with straightforward conduct of what deserves to be done is not always simple, but it can be worthwhile.

Finally, this has been a year of particularly able and cordial teamwork on the part of Association staff members. For their help, and for the unfailing cooperation I have had from other officers, I am most grateful.

PAUL L. WARD, *Executive Secretary*

REPORT OF THE MANAGING EDITOR FOR 1968

With one exception, statistics would indicate that the past year was much like any other year in the annals of the *American Historical Review.* The twenty-three articles published in Volume LXXIII include one on ancient history, three on medieval history, nine on modern European history, seven on United States history, one on Latin American history, and one on Asian history, in addition to a general article and Professor Hajo Holborn's presidential ad-

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dress, "The History of Ideas." Articles received between September 1967 and September 1968 numbered 251, as against 232 in the preceding year; of those 251, 196 were returned, 12 were accepted, and 43 were still under consideration on September 1. A total of 1,058 reviews were published.

The figure that suggests a significant statistical departure is the rise in total number of pages from 1,635 in Volume LXXII to 1,772 in Volume LXXIII; or, if I may borrow from the current volume to put it in a still more dramatic way, both the October and December issues now in your hands have as many or more pages than the largest number in the last of the four-issue volumes. The move to five issues initially meant a saving both in size and in printing costs; those gains have been more than wiped out. To some extent, this growth in size reflects somewhat more flexibility than in years past in the length of articles and in the assigned length of reviews, though candor as well as prudence demands that I report no greater willingness on the part of the editors to look kindly on reviews that exceed the assigned wordage. The major factor is a publishing explosion. In Volumes LXVIII and LXIX there were slightly under 800 reviews; Volumes LXX and LXXI leveled out at 966 and 967; in the past year, however, nearly a hundred more reviews were printed, and it should be remembered that we review only about a third of the books received and that a second third goes to swell the lists at the end of four out of the five issues. The discretion of the editors can help by a judicious (and one hopes stimulating) linkage of books on similar themes and by greater flexibility as to length. But, unless the *Review* were to abandon its intention to be as comprehensive as possible—an intention that makes the *Review* unique among historical periodicals and that is emphatically not otherwise served, even by the valuable multiplication of specialist journals—there is only the prospect of continued growth as we begin more systematically to tap the scholarly output of presses in parts of the world, which so far we have sampled only arbitrarily and, often, in comparative ignorance. Soon this prospect will force the *Review* to a fundamental reconsideration of its finances, its staff, and its internal and external organization. But I say this only as a John the Baptist, crying in the wilderness to prepare the way; I have as yet no message of salvation.

In another sense, the past year has been anything but ordinary. On June 1, after some months in on-the-job training, I took over formal responsibility for the *Review* from Henry R. Winkler. It would ill become me to suggest that his ascent to a deanship and my succession to his editorial post are further illustrations or results of the rise of the bourgeoisie. I can, however, with considerably more confidence, apply another cliché from the historian's arsenal: it is indeed an age of transition. But the transfer of power from one English historian to another should suggest that one look for continuity rather than revolution, and it is not only fair but important to say that, almost without exception, what changes you may see in the *Review* over the next two to five years were being considered before Mr. Winkler left and first came to my mind as practicable possibilities in long and rewarding conversations with him. We have too an invaluable element of continuity in our senior editorial staff, Patricia M. Fox, M. Rita Howe, and John T. Appleby. Bound as they are by the triple vows of

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poverty, accuracy, and discretion, they have brought to the new Managing Editor, as they did to the old, an unparalleled knowledge of the profession and a dedication that are priceless assets. It should be noted, too, that their labors have this year brought to completion the imposing ten-year index to the *Review* for the period 1955–1965. Nor can a fledgling editor omit a tribute to a hard-working, imaginative, and sensible Board of Editors, who have kept me, on more than one occasion, from falling into the ditch. This year marks the expiration of the term of Professor Gordon Craig, whose liberation is accompanied by our sincere thanks for the efficacy with which, over the past five years, he has dealt with the vast field of modern European history.

One more comment is needed: The disparity cited earlier in this report between the 251 articles submitted and the tiny number accepted suggests certain wastage of effort by authors, readers, and editors. Some slippage is, of course, inevitable, and the sanity of no editor could survive an almost exact correspondence of submissions and acceptances. But nearly half of those articles submitted can be rejected on the Managing Editor's own motion as entirely inappropriate to the *Review*. It should therefore be said again that the *American Historical Review*, with a spread of readership perhaps unparalleled by any other historical journal, must aim at a significant degree of generality in the articles it publishes. It is emphatically not the vehicle for one specialist to address another on a narrow topic of little or no interest outside that specialty; nor do we want mere generalities. What is appropriate to the *Review* is a truly scholarly article transcending any seeming narrowness in its subject, which crosses the boundaries of disciplines or of nations and can stimulate productive interest or controversy outside as well as within its immediate range and which can prove to nonspecialists the excitement inherent in a specialty treated with sufficient breadth and imagination. We receive many such articles and would like to receive more; the genre is by no means languishing. But an author may not think immediately of the *Review*, perhaps because a specialized journal more likely occurs to him first or because he assumes, incorrectly, that publication in the *Review* will be long delayed. If this report helps to stimulate the submission of such articles and to discourage the submission of more narrowly conceived articles, however useful, then it will have gone far toward ensuring the continuation of the *Review* as the general voice of a distinguished profession.

R. K. Webb, *Managing Editor*

REPORT OF THE TREASURER FOR THE PERIOD BEGINNING SEPTEMBER 1, 1967, AND ENDING JUNE 30, 1968

This report covers a period of ten months, rather than one year, owing to the fact that the Association's business year, to conform to general usage, has now been changed to begin July 1, rather than September 1 as heretofore. While not completely realistic, the budget figures stated represent five-sixths of the figure originally set for a twelve months' period. Our budget for 1967–1968 was a deficit one. Our excess of revenue over expenses for the ten-month period is $8,486.49, as against an expected deficit of $16,500.

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In accordance with the instructions of the Council, income from permanent investments has been allotted to each of the several funds on a uniform basis.

The headquarters and equipment, less the standard depreciation, are valued at $197,495.12 as against $147,294.66 in 1967, the increase resulting from acquisition of a new piece of property at 404 A Street, S.E.

The permanent investments in the regular account, the proceeds from which are available for the general purposes of the Association, amount to $391,646.71 at cost (market value being $590,639.66) as against $394,542.88 (market value being $570,916.08) in 1967. The total increase in market value amounts to $19,723.58 for the period.

Special funds and grants and endowments for specified purposes amount to $474,524.26 as against $441,943.56 in 1967. The market value of permanent investments in the regular account increased $12,621.29. In the Matteson Fund market value decreased $2,083.98.

Funds, restricted and unrestricted, and the plant fund comprising the total assets of the Association amount to $1,133,802.60 as against $1,101,014.59 in 1967. This relatively modest increase is due largely to funds held as grants and subject to steady disbursements as the work of projects goes along.

A condensed account of the Association's financial operations during the past fiscal year will appear in the Association's *Annual Report*. All financial accounts have been audited by Main, Lafrentz & Company, certified public accountants. Their report is on file at Association headquarters, where it is available for inspection by interested members. Filed also at headquarters and available for examination is the report of the Fiduciary Trust Company, approved by the Chairman of the Board of Trustees, on the securities held in its custody.

ELMER LOUIS KAYSER, *Treasurer*

MINUTES OF THE COUNCIL MEETING OF THE AMERICAN HISTORICAL ASSOCIATION THE STATLER HILTON HOTEL, NEW YORK CITY DECEMBER 27, 1968, 9:30 A.M.

The meeting was called to order by the chairman, AHA President John K. Fairbank. Present were Mr. Fairbank; C. Vann Woodward, Vice-President; Elmer Louis Kayser, Treasurer; Paul L. Ward, Executive Secretary; R. K. Webb, Managing Editor of the *American Historical Review;* voting former President Frederic C. Lane; and elected members Carl E. Schorske, Thomas C. Cochran, John L. Snell, Jr., William B. Willcox, Caroline Robbins, David M. Potter, and Lynn White, jr. Also present at the Council's invitation were Robert R. Palmer and Robert L. Zangrando, Assistant Executive Secretary. Elected member Philip D. Curtin and voting former Presidents Hajo Holborn and Roy F. Nichols were unable to attend.

The minutes of the Council meeting of September 28, 1968, were approved.

Two items of interim action taken by the Executive Committee since the Council met on September 28 were approved. The Association is auth-

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orized to enter into a contract with the Office of Education, through Indiana University, for a project in history education, subject to detailed negotiations as to financial particulars. Also, on matters of professional ethics, it was decided that "When any request is made to the Association to take action with respect to some alleged breach of ethics within the profession, the Executive Secretary will limit his response to ascertaining the facts and mediating informally, to the extent that he judges may be useful and welcomed. His special interest will be to promote clarity on any blurred points of professional ethics, while seeing that other authorities (as appropriate) handle any adversary proceedings or any decision on penalty. The *AHA Newsletter,* at the discretion of its editor, will welcome any statement or brief article from an interested party if it contributes to such clarification and avoids being appreciably equivalent to hostilities or a penalty."

The Council next approved the establishment of a liaison committee of the American Historical Association and the Association for Asian Studies. Mr. Fairbank submitted brief minutes of the committee's meeting on December 11, 1968, including a list of six lines of prospective liaison activity that he felt might guide similar liaison committees whenever such committees were needed.

Mr. Webb commented that he was thinking of arranging a gathering of all the editors of the various historical societies at the next Annual Meeting in an attempt to achieve better coordination. Mr. Fairbank responded that this gathering would have the Council's blessing.

Mr. Webb presented the report of the Managing Editor, along with a supplementary memorandum to alert the Council as to prospective changes in the organization of the *Review.* After members had examined this, Mr. Kayser moved that the Council authorize the Managing Editor to add to his staff an experienced proofreader, to begin negotiations with the Macmillan Company to terminate the contract now in force, and to add staff as needed to cover any functions thereby taken over. In response to a question, Mr. Webb explained that the Council had authorized him to approach Mr. Roger Shugg for professional advice on this matter. Both Mr. Kayser and Mr. Ward spoke in favor of terminating the present contractual relationship. As Treasurer, Mr. Kayser felt it would be a step toward progressive strengthening of the *Review.* The contractual arrangements made in 1895 and in 1899 are now inappropriate. The details of the matter, Mr. Webb pointed out, might be modified by Mr. Shugg's report, and the Council would receive full information on this in the future. The Council voted to accept the motion.

An oral report from the Assistant Executive Secretary contained information on four committees. In connection with the Committee on Teaching, he mentioned that, because Mr. Webb is now editing the Service Center's pamphlet series, more work can be done on other AHA projects. The Committee on University and College Teaching held a two-day meeting earlier in December, along with nine resource people, to review the condition of history in junior and community colleges. The Professional Register, Mr. Zangrando said, had been entirely revised this fall to bring the system up to date. Discussions had been held with the US Employment Service to see what help it might give to the Association in placement, and two officers and committee members examined the USES opera-

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tion at the annual meeting of the American Political Science Association. Under the supervision of the Committee on Quantitative Data in History, the project on non-US history, funded by the National Endowment for the Humanities, has progressed successfully under the direction of Jacob Price at the University of Michigan, and efforts are now under way to have the working papers of the project published.

The next matter discussed was a memorandum from the Committee on Teaching on the desirability of a journal to review teaching materials in history and to publish articles on related issues. Mr. Zangrando explained that the initial plans for such a journal resulted from collaborative efforts of the AHA and the Organization of American Historians. It was suggested that the Committee on Teaching further investigate possible cosponsorship of the project with a journal now existing. A motion was made and passed to this effect.

Mr. Snell proposed the following motion which was adopted by the Council:

Resolved, That the Council, having taken note of Robert Zangrando's decision to resign his post as Assistant Executive Secretary in 1969, expresses its thanks to him for the imaginative and energetic services he has rendered to the Association and extends its best wishes to him in his future professional activities.

The next item discussed by the Council was the report of the Committee on Committees. A motion was made and passed to add the Managing Editor as an ex officio member of the Committee on Teaching, in view of his responsibility for the pamphlet series.

Mr. Palmer questioned whether the appropriateness of having two people from the same university on a committee was a policy question for the Council to decide. Mr. Schorske thought that a more important question is not a man's university but rather his role. He thought that any committee should include people on every side of a basic, serious issue in order to obtain a plurality of views. On the other hand, he did not see how anyone who represents a strong radical view or takes a very nongovernmental position could make a positive contribution to the Committee on the Historian and the Federal Government. Mr. Ward acknowledged that experience with government had been the chief consideration in making up the committee. Mr. White thought it unwise to change the names, and he urged the Council to confirm the committee. At the same time he expressed the view that not just experience, but also fundamental attitudes, be considered in the future in this matter of the relation of the Association and the government. Mr. Schorske then moved that the committee should represent the full span of opinion within the Association on the relationship of the organized historical profession and the federal government. The motion was adopted.

Mr. Webb noted that there was a vacancy on the Board of Editors. For that place, vacated by Gordon Craig, he nominated Peter Gay. This motion was passed by the Council.

Mr. Snell moved, as suggested by the Committee on Committees, that the Service Center Editorial Advisory Board be discontinued. The motion was passed.

The Council noted that the Committee on University and College Teaching did not represent any small colleges. It was pointed out that new thought on teaching was coming from colleges rather than universities. It was moved

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and passed that the Executive Committee add a person to the committee to represent colleges.

The next matter taken up was the report of the Committee on Ph.D. Programs in History. A lengthy discussion followed on the problems of publishing a list of institutions granting the Ph.D. in the next *AHA Newsletter*. Mr. Fairbank offered for discussion a possible resolution giving grounds for withholding publication. Mr. Snell argued that the AHA should go ahead with the publication of the list on the grounds that we owe information to prospective doctoral students and to departments having graduate programs; the publication of such lists will help them strengthen their Ph.D. programs. Mr. White, recalling his own experience in helping launch the present accrediting agency on the West Coast, urged that evaluation be by substantial committees going directly to institutions to examine them every five years, even though such a course would involve inconvenience and great labor and pressures. He proposed a substitute version of the resolution. The Executive Secretary was asked if he received many objections to the publication of this list and, if so, how widespread they were. Mr. Ward informed the Council that, while several institutions felt that publication of such a list would prove embarrassing, he was unable to judge the extent of the repercussions. Mr. Schorske reported two contrasting experiences, one at an institution where the influence of the AHA committee's program was clearly useful, and another where it seemingly discouraged innovation and progress. Mr. Fairbank stated that the hard work of the committee was appreciated, but that much more work appeared to be needed. He assured members that the minutes of the discussion would be given to the committee. The Council then voted to accept Mr. White's resolution as follows:

1. The graduate student public deserves to be protected against involvement in inadequate Ph.D. training. Such protection can be provided by making information available concerning inadequacy of Ph.D. programs.

2. The AHA should not at this time get into the business of accreditation which would necessarily involve committees of visitation.

3. The Council is very appreciative of the hard work and care with which the Committee on Ph.D. Programs in History has pursued its task of securing information.

4. The Council believes that at present the committee can achieve its best influence if it avoids the publication of its lists of institutions since this act would constitute *de facto* accreditation. The Council cannot authorize the committee to speak publicly on this matter in the name of the AHA.

5. The Council believes that the committee should pursue its work without publicity by communicating the results of its deliberations to the institutions concerned. The Council suggests that a report on the work of the committee should appear in the *Newsletter*.

Mr. Fairbank resumed the meeting after lunch with a discussion of certain details in the Committee on Committees' report. Mr. Ward reported that the terms of the Beer bequest require that the Beer Prize be awarded every year. The proposal was then approved that the same committee that awards the Adams Prize determine the recipient because the two overlap.

The next recommendation of the Committee on Committees was for the dis-

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charge of the Committee on the Freedom of Historical Inquiry, whose chairman, Arthur Bestor, has had to resign. Mr. Snell thought that it might be important to preserve this committee in a "watchdog" function. It was pointed out that the AHA has such a committee and that the make-up of the present committee was focused on the problems of copyright. It was agreed accordingly to discontinue the Committee on Freedom of Historical Inquiry, on the grounds that its concerns can best be handled as a staff matter.

The report of the Committee on Bibliographical Services to History contained a motion to change the name of this committee to "Committee on Information Services."

Mr. Ward offered a motion to establish a standing Committee on Research Needs and Opportunities that would review matters called to its attention under this head and on its own initiative and that would prepare a brief annual report on some specific and limited aspect of its subject. Such a committee would be composed of three members appointed by the Council for three-year terms, and the President, Vice-President, Executive Secretary, Managing Editor, and the most recent Program Chairman ex officio. The Council voted to accept the motion.

It then voted to accept the report of the Committee on Committees as a whole, approving the membership of committees as follows:

Standing Committees:

Committee on Committees.—Paul L. Ward, American Historical Association, chairman (ex officio); Elizabeth Foster, Bryn Mawr College; Charles O. Hucker, University of Michigan;* Richard Leopold, Northwestern University;* John A. Schutz, University of Southern California; John K. Fairbank, Harvard University (ex officio); Elmer Louis Kayser, George Washington University (ex officio); R. K. Webb, *American Historical Review* (ex officio); C. Vann Woodward, Yale University (ex officio).

Commitotee on the Harmsworth Professorship.—Bell I. Wiley, Emory University, chairman; D. E. Fehrenbacher, Stanford University;* T. Harry Williams, Louisiana State University; John K. Fairbank, Harvard University (ex officio); C. Vann Woodward, Yale University (ex officio).*

Committee on the Historian and the Federal Government.—Bradford Perkins, University of Michigan, chairman; Henry David, National Research Council; Sidney Fine, University of Michigan;* William M. Franklin, Department of State; Joe B. Frantz, University of Texas;* David Landes, Harvard University; Rowland L. Mitchell, Social Science Research Council; Donald Swain, University of California, Davis.

Committee on Honorary Members.—Julian P. Boyd, The Papers of Thomas Jefferson, chairman; John K. Fairbank, Harvard University;* Hajo Holborn, Yale University; Frederic C. Lane, Westminster, Massachusetts; Roy F. Nichols, University of Pennsylvania.

Committee on Information Services.—Aubrey C. Land, University of Georgia, chairman; Lee Benson, University of Pennsylvania; Howard F. Cline, Hispanic

* New member this year.

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Foundation; R. Stuart Hoyt, University of Minnesota; Martin Ridge, *Journal of American History*;* Henry R. Winkler, Rutgers University;* R. K. Webb, *American Historical Review* (ex officio).

Committee on International Historical Activities.—Paul L. Ward, American Historical Association, chairman (ex officio); Robert F. Byrnes, Indiana University;* Howard F. Cline, Hispanic Foundation; Rosalie L. Colie, University of Toronto; John S. Galbraith, University of California, Los Angeles;* Helmut Koenigsberger, Cornell University; Frederic C. Lane, Westminster, Massachusetts; Boyd C. Shafer, Macalester College; Kenneth Stampp, University of California, Berkeley; Arthur Wright, Yale University.

Committee on Ph.D. Programs in History.—E. David Cronon, University of Wisconsin, chairman; Robert D. Cross, Hunter College; Howard Lamar, Yale University;* J. Russell Major, Emory University;* David A. Shannon, Rutgers University;* Lacey B. Smith, Northwestern University; John L. Snell, Jr., University of North Carolina, Chapel Hill; Theodore Von Laue, Washington University.

Committee on the Professional Register.—Robert L. Zangrando, American Historical Association, chairman (ex officio); Charles Blitzer, Smithsonian Institution; Lawrence A. Cremin, Teachers College, Columbia University; Henry Hill, University of Wisconsin;* Benjamin A. Quarles, Morgan State College; Roger W. Shugg, University of New Mexico;* Elmer Louis Kayser, George Washington University (ex officio).

Committee on the Program (standing committee component).—John William Ward, Amherst College, chairman; Raymond Grew, University of Michigan; Richard Schlatter, Rutgers University; James E. Sheridan, Northwestern University; Lawrence W. Towner, Newberry Library; Robert R. Palmer, Princeton University (ex officio); Paul L. Ward, American Historical Association (ex officio); C. Vann Woodward, Yale University (ex officio); two members to be appointed later.

*Committee on Research Needs and Opportunities.***—C. Vann Woodward, Yale University, chairman (ex officio); Robert R. Palmer, Princeton University (ex officio); Lawrence W. Towner, Newberry Library (ex officio); Paul L. Ward, American Historical Association (ex officio); R. K. Webb, *American Historical Review* (ex officio); three members to be appointed later.

Committee on Teaching (Service Center for Teachers of History).—Thomas Pressly, University of Washington, chairman; Merle Borrowman, University of Wisconsin;* Donald Cole, Phillips Exeter Academy; Nelda Davis, Prince George's County Schools; Henry Drewry, Princeton University; Edwin T. Fenton, Carnegie-Mellon University; Joyce Fulton, Woodside High School, San Mateo, California; Jack P. Greene, Johns Hopkins University; Louis Harlan, University of Maryland; Paul S. Holbo, University of Oregon; John Teall, Mt. Holyoke College;* R. K. Webb, *American Historical Review* (ex officio).*

Committee on University and College Teaching.—Ralph E. Morrow, Washington University, chairman; Richard M. Douglas, Massachusetts Institute of Tech-

* New member this year.
** New committee this year.

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nology; Louis G. Geiger, Colorado College;* J. Joseph Huthmacher, Rutgers University;* Robert W. Johannsen, University of Illinois; William R. Taylor, State University of New York, Stony Brook; David Trask, State University of New York, Stony Brook.*

Prize Committees:

Committee on the Herbert Baxter Adams Prize and the George Louis Beer Prize.—Hanna H. Gray, University of Chicago, chairman; John Spielman, Haverford College;* Peter Stansky, Stanford University;* Peter Stearns, Rutgers University;* Henry A. Turner, Yale University.

Committee on the Albert J. Beveridge Award and the John H. Dunning Prize.—Jack P. Greene, Johns Hopkins University, chairman; Norman A. Graebner, University of Virginia; Ramón Eduardo Ruiz, Smith College; Robert H. Wiebe, Northwestern University;* J. Harvey Young, Emory University.

Committee on the Clarence H. Haring Prize.—J. H. Parry, Harvard University, chairman; Robert Burr, University of California, Los Angeles; Richard Morse, Yale University.

Committee on the Littleton-Griswold Fund.—Joseph H. Smith, Columbia University Law School, chairman; Gerald Gunther, Stanford University; Michael Kammen, Cornell University; Alfred Kelly, Wayne State University; Leonard Levy, Brandeis University;* Paul Murphy, University of Minnesota.

Committee on the Robert Livingston Schuyler Prize.—Willson H. Coates, University of Rochester, chairman; Philip Curtin, University of Wisconsin; Philip P. Poirier, Ohio State University; David Spring, Johns Hopkins University; Sylvia Thrupp, University of Michigan.

Committee on the Watumull Prize.—Stanley Wolpert, University of California, Los Angeles, chairman;* Ainslie T. Embree, Duke University; Robert E. Frykenberg, University of Wisconsin.*

***Ad Hoc* Committees:**

Committee on American-East Asian Relations.—Ernest May, Harvard University, chairman; Alexander DeConde, University of California, Santa Barbara; John K. Fairbank, Harvard University; Norman A. Graebner, University of Virginia; Richard W. Leopold, Northwestern University.

Committee on the Commemoration of the American Revolution Bicentennial.—Lester Cappon, Institute of Early American History and Culture, chairman; John R. Alden, Duke University; Whitfield Bell, American Philosophical Society; Julian P. Boyd, The Papers of Thomas Jefferson; Lyman H. Butterfield, Massachusetts Historical Society; Hugh F. Rankin, Tulane University; Otis Singletary, American Council on Education; William J. Van Schreeven, Archivist of Virginia; Clarence L. Ver Steeg, Northwestern University.

Committee on the Feature Films Project.—J. Joseph Huthmacher, Rutgers University, chairman; William H. McNeill, University of Chicago; Michael Petro-

* New member this year.

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vich, University of Wisconsin; Donald H. Shively, Harvard University; Leo Solt, Indiana University; Bayrd Still, New York University.*

*Committee on the History of Education Project (advisory).***—Thomas J. Pressly, University of Washington, chairman; Robert R. Palmer, Princeton University; Charles G. Sellers, University of California, Berkeley; B. O. Smith, University of Illinois; Leo F. Solt, Indiana University; William R. Taylor, State University of New York, Stony Brook; Theodore Von Laue, Washington University; Phillip H. Woodruff, Westport Public Schools.

Committee on Quantitative Data in History.—Lee Benson, University of Pennsylvania, chairman; William O. Aydelotte, State University of Iowa; Allan Bogue, University of Wisconsin; Thomas Condon, American Council of Learned Societies; David Herlihy, University of Wisconsin; Val Lorwin, University of Oregon; William P. McGreevey, University of Georgia; Warren Miller, Inter-university Consortium for Political Research; Rowland Mitchell, Social Science Research Council; Jacob Price, University of Michigan; Theodore Rabb, Princeton University; Henry Rosovsky, Harvard University; Leonard Thompson, University of California, Los Angeles; Robert L. Zangrando, American Historical Association (ex officio).

Joint Committees:

Joint Committee of the Canadian Historical Association and the American Historical Association.—John Hall Stewart, Case Western Reserve University, US chairman; Alice Stewart, University of Maine; Russel B. Nye, Michigan State University.*

Joint Committee for the Defense of the Rights of Historians under the First Amendment.—AHA members: Alfred Kelly, Wayne State University;* Paul L. Ward, American Historical Association (ex officio); C. Vann Woodward, Yale University (ex officio).*

George W. Pierson of Yale University was reappointed a delegate to the American Council of Learned Societies. Jim B. Pearson of the University of Texas was named to the Executive Board of *Social Education,* a periodical published by the National Council for the Social Studies. Arthur S. Link of Princeton University was appointed to the National Historical Publications Commission. Philip D. Curtin of the University of Wisconsin was named to a second term on the Social Science Research Council, to begin in January 1970.

Mr. Fairbank then presented a motion, which the Council accepted, as follows:

WHEREAS, The AHA Constitution has contained statements concerning aims and structure of the organization and other statements concerning deadlines for ballots and amounts of dues in dollars, at entirely different levels of operational significance; therefore

It is now voted that the Executive Committee be asked to prepare a list of bylaws in which should be extracted from the Constitution various matters of operational significance, only said bylaws to be submitted to the Council and eventually for adoption by the membership, together with the revised text of the Constitution according to the procedure provided therein.

* New member this year.
** New committee this year.

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The Council then acted upon a recommendation by the Nominating Committee for 1968 and voted to lay before the Business Meeting a constitutional amendment changing the deadline for the receipt of ballots from December 20 to December 15.

Mr. Ward suggested that the Council take the action mentioned in the announcement in the *AHA Newsletter* (VII [Oct. 1968], 5), in connection with the constitutional amendment as to dues, action to be contingent on the passing of this amendment. Two additional dues categories were proposed reducing dues to $7.50 for persons having formal academic retirement status as a result of either age or disability (who would receive only the *Review* and the *Newsletter*), and for spouses of AHA members (who would receive the *Newsletter*, the program of the Annual Meeting, the *Annual Report* if requested, the opportunity to join the Professional Register, and the right to vote for nominees for President, Vice-President, Treasurer, the Council, and the Nominating Committee—everything except the *Review*).

The Council voted to establish these new categories of dues as of July 1, 1969, if the necessary constitutional amendment is passed.

The next point on the agenda was the Treasurer's report, at which time Mr. Fairbank intervened and read the following resolution, already adopted by unanimous mail vote of all members of the Council, the Treasurer excepted:

> WHEREAS, Elmer Louis Kayser has since 1958, in his capacity as Treasurer, so greatly contributed to the day-to-day management and long-term development of this Association, the other members of the Council wish in this way and while he is still in midpassage to record their appreciation of the foresight, prudence, and energy with which he is currently serving the Association.

Mr. Kayser pointed out that his report as of June 30 showed hardly any operating surplus. The explanation was that recent balances, by Council action, had gone to buy essential real estate. Economies were now in order.

Mr. Fairbank called for adopting three points presented in Mr. Kayser's oral report, as follows:

1. That no overdraft be permitted on any budget account unless requested in writing, covered in amount by the transfer of an unexpended amount from another budget account, and approved by the Finance Committee.
2. That the Executive Secretary and the Treasurer be directed to report to the Council at its next annual meeting the extent to which suggestions and recommendations for economy have been adopted during the year and the effect of these on the financial condition of the Association.
3. That there be included in the minutes of this meeting in the form required by the bank a resolution authorizing the proper officers of the Association to borrow a sum not to exceed twenty-five thousand dollars for a period of not more than six months and to sign the necessary documents in connection with such a loan, the amount and duration of which, within the limits set, to be determined by the Treasurer.

Gerald White, President of the Pacific Coast Branch, joined the Council meeting briefly as a guest and reported informally on the Sixty-first Annual Meeting of the Branch and on the new arrangements for the *Pacific Historical Review*, arrangements made possible by the generosity of the University of California, Los

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Angeles. Mr. Lynn White, jr., commented that, even though the Branch is small, it is in good health.

After a brief recess, Mr. Fairbank suggested setting the time of the spring meeting of the Council. The Council decided on Saturday, April 12, 1969, at 9 a. m. at the AHA offices.

The next item was approval of the revised budgets for 1968–1969, and the preliminary budget for 1969–1970, presented jointly by the Executive Secretary and the Treasurer. Mr. Webb noted that the *Review*'s part of the budget will look different at the April meeting. It may involve raising salaries, but will not mean a radical change in the budget because additional funds will become available when the *Review* becomes its own publisher. Mr. Ward remarked that the income from dues was estimated conservatively. It was moved and passed that the Council approve the budget.

The next item discussed was the Executive Secretary's report, prepared for delivery at the Business Meeting. It was moved and passed that the Council accept this report.

Mr. Ward commented that Messrs. Sidney Fine and Norman Graebner had been appointed as AHA nominees to the Archivist's Advisory Council and that this council at its first meeting a few weeks earlier had, he understood, made a promising beginning on matters important to historians. Copies of the printed "Report of the Joint Committee on the Status of the National Archives" were passed out, and Mr. Ward mentioned having been in communication with Senator Scott's office on a projected bill for an independent archives authority.

The Council next considered a protest made by Mr. Gabriel Kolko, who urged that the Association should formally disassociate itself from the recommendations published in the October 1968 *Newsletter* by a National Research Council committee and entitled "The Behavioral Sciences and the Federal Government." Mr. Ward laid before the Council a statement explaining the limited character of the AHA's affiliation with the NRC and added that the recommendations in question were contained in a report that had not even been presented to the NRC for endorsement. It was moved and adopted that the Executive Committee should decide whether to refer the issues raised by Mr. Kolko to the present standing committee or to create an *ad hoc* committee.

Under old business, brief attention was given to information from Mr. Zangrando on arrangements at New Orleans for the Annual Meeting in 1972.

The Council next approved the text of the following resolution to be sent to the outgoing and incoming Secretaries of State:

> The American Historical Association has long been concerned that the Department of State keep to a reasonable minimum the time lag between origin of its significant documents and their publication in the *Foreign Relations of the United States*. In the three years since the Association last voiced this concern forcefully in the *AHA Newsletter*, the time lag regrettably has further increased, from twenty-one years to twenty-three years. Now, given the great volume of documents for 1945 and 1946, an additional increase to twenty-five years is threatened. The Association therefore owes it to the importance of this documentary record of our nation's foreign policy to express its special reasons for urging that the Historical Office be provided without delay with more manpower and money for the task.

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First, the *Foreign Relations* series has progressed to the point in past time where its volumes begin to deal with postwar problems that are still now major problems. Our nation has a proud tradition of being an open society, whose citizens contribute to the updating of public opinion and to government policy on the basis of uniquely generous access to the facts. The Association is concerned that the historical record continue to play its full part, through the *Foreign Relations* series, in sustaining the level of civic discourse on our foreign affairs.

Second, the Association attaches importance to the department's established provisions for availability of documents in the "restricted period" between publication in the *Foreign Relations* series and the opening of access under the thirty-year rule. Since the opening of the department's files to restricted access by scholars depends on prior publication in a *Foreign Relations* volume, the further the series falls behind currency the greater the loss to historical studies at this level of active research.

Third, the Association very much appreciates the quality of work currently maintained by the *Foreign Relations* staff and fears for its undermining if a further falling behind currency ends by injuring morale. The achievement of publishing a past year's documents in a year, if made possible by an increase of staff, would not only strengthen morale; it would empower the staff to establish a schedule and bring about adherence to it, with offices in the department and the Government Printing Office cooperating. Close control of reasonable delays, now all too common because of lack of an effective schedule, is required by the importance of the work.

Fourth, the Association considers it eloquent that in 1967 the *Foreign Relations* staff had only thirteen historians, as against sixteen twenty years before; whereas the bulk of documents to be examined in 1967, if a year were to be done in a year, was nearly twice what it had been in 1947. The contrast between the activity of the department in the two years of origin (which for 1967 was a postwar year and for 1947 a depression year) indicates the real terms of the problem now.

The Association feels a simple duty, to the historical profession and all those who turn to historical evidence to improve their judgments of foreign policy, to urge that the Secretary of State accept the goal of bringing the *Foreign Relations* series back to twenty years from currency and authorize the additional manpower needed to achieve this goal.

The Executive Committee's memorandum on AHA functions, available to any members who wished it at the Business Meeting, was next discussed, and its wording on international historical contacts was revised. The Council then voted to accept the memorandum as changed.

The first item of new business was the report of the Committee on International Historical Activities (CIHA) and the resolution proposed by Mr. Kenneth Stampp on the issue of participation in the Moscow Congress. Mr. Ward said that on the preceding evening he had telephoned Mr. Byrnes, who said he did not have any fresh information to give to the Council.

Boyd Shafer, Vice-President of the International Committee, then joined the meeting at the Council's request. Mr. Shafer felt that the AHA, while still continuing its preparation for participation in the Moscow Congress, should keep the situation under close review, recognizing the difficulties involved but realizing also the importance of comradeship with historians everywhere.

Although the British national committee had decided not to participate in the Moscow Congress, he pointed out, the American national committee (CIHA), under the chairmanship of Mr. Ward, voted six to two to protest actions in Czechoslovakia and to continue preparations for the meeting.

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He was sure that no one approves or condones Soviet measures in Czechoslovakia. Arguments for and against the meeting were presented by various members of the Council. Mr. Shafer said that it was not posssible to shift the meeting, but that we were free to protest the Soviet action.

Mr. White suggested the following motion be presented at the Business Meeting, along with the Stampp resolution:

WHEREAS, USSR scholars and scientists have attended conferences in the USA during the period of American involvement in Vietnam despite the fact that public opinion in Communist nations disapproves passionately of American action there; and

WHEREAS, The contact of USSR intellectuals with their peers in non-Communist lands may assist an eventual resolution against intellectual repression in Russia and against actions like the invasion of Czechoslovakia; therefore,

Be It *Resolved,* That the AHA continue its cooperation with plans for the Moscow meeting of the International Historical Congress in 1970.

Mr. Schorske remarked that experience of two previous international conferences in Stockholm and Vienna had shown that sessions became impossible when politics intruded, but that otherwise it often had solid value. He thought that mention of the Vietnam war in the resolution would not be wise.

Mr. Potter felt that we ought to participate in the congress and not boycott it. He made the following motion:

WHEREAS, Opposition has been expressed to participation of the American Historical Association in the Moscow Congress,

It is *Resolved,* That the Association is not indifferent to this opposition, nor to the invasion of Czechoslovakia. However, this Association is of the opinion that it ought not to withdraw from the Moscow meeting, but ought to approach it as an international congress of historians which, although it happens to be held in Moscow, is not under the direction of, nor answerable to, the Soviet government.

Mr. Snell suggested a motion to accept Mr. Potter's resolution, and it was accepted.

It was moved and passed to approve the following statement on UNESCO drafted by Mr. Shafer, to meet a request from the US National Commission on UNESCO:

UNESCO, through its support of cultural and educational activities, has promoted collaboration among the historians of the nations and has thus furthered the scholarly work of historians of all countries, including those of the United States.

The subventions of UNESCO have made international cooperation among historians possible on a scale larger than ever before. These subventions, totaling $172,765.00 since 1950, have enabled:

(1) the holding of international congresses of historians every five years (Paris, 1950; Rome, 1955; Stockholm, 1960; and Vienna, 1965), congresses at which fifteen hundred to three thousand historians from about the world have met and exchanged ideas; (2) many meetings of twenty-two scholarly groups, affiliated as external or internal commissions with the International Committee of Historical Sciences, in various fields of history (lists of UNESCO-supported activities in history on file);
(3) the publication of essential work such as the multivolumed *Rapports* containing the

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many papers and acts of the international congresses, the annual volumes of the *International Bibliography on Historical Sciences,* and numerous scholarly indexes, glossaries, and documentary volumes (lists on file);
(4) the creation of formal and informal scholarly ties between and among the national committees of forty-four nations; and
(5) the forming of friendships and the stimulation of dialogue among hundreds of individual historians throughout the world.

Scholarship is advanced both by the accumulation of knowledge that takes place throughout the world and by encounters of diverse interpretations held by historians in different countries. UNESCO, through its material aid and moral support, has assisted in the accumulation and provided for encounters.

UNESCO's support has, then, enabled the building of international bridges across which have flowed ideas enriching historical understanding. Indeed, without UNESCO support those cultural relationships indispensable for the enrichment of any discipline could scarcely have been established in historical study, for historians themselves have not had the resources to provide for international meetings and publications.

The American Historical Association wishes, therefore, to support the continuation of UNESCO's cultural and educational activities. Further, the Association hopes that UNESCO will be able to expand those activities so that historians as well as other scholars may further contribute to the world's knowledge and to the cause of international understanding.

Mr. Cochran moved that the Executive Committee in the future be constituted as follows: the President, the Vice-President, the President of the preceding year, the Executive Secretary, the Treasurer, and the Managing Editor. It was thought desirable to include on the committee one elected member of the Council. Mr. Cochran said that in this case, he suggested deleting the President of the preceding year since the total membership is limited to six by the Constitution. Mr. Cochran suggested that Mr. Snell be named.

The motion was adopted with this revision, and another motion was passed to name Mr. Snell to the committee.

Mr. Fairbank moved on to consider a protest, lodged by historians at the University of Kansas, against the current presentist emphasis in the policy statements of the National Endowment for the Humanities. Mr. Ward was asked to secure a copy of the endowment's reply and then to inform Mr. Barnaby Keeney to express the Association's concern.

On the subject of the recent reduction of Fulbright awards, the following resolution was moved and adopted:

Resolved, That the American Historical Association views with deep concern recent cuts by the federal government in the Fulbright program and other similar programs of international scholarly exchange and cooperation. It strongly urges that the new administration and Congress restore such cuts in the interest of the United States' role of leadership in the world.

Mr. Cecil Fitzhugh Gordon was re-elected to a further term on the Board of Trustees.

Because of time limitations, the appointment of panel members to review the advanced history test, which had been requested by those responsible for the Graduate Record Examinations, was left to the Executive Secretary.

Mr. Snell introduced the subject of Negro history. He thought that the Asso-

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ciation should appoint a committee representing a wide spectrum of views to investigate the teaching of Negro history in schools from the earlier grades through the university level. Mr. Schorske thought that this proposal was long overdue.

Mr. Snell moved that the Executive Secretary be authorized to appoint such a committee, after a preliminary inquiry directed toward people in the field, without further approval of the Council. The motion was adopted.

Having finished its business, the Council adjourned its meeting at 5:30 p.m.

PAUL L. WARD, *Executive Secretary*

MINUTES OF THE BUSINESS MEETING OF THE AMERICAN HISTORICAL ASSOCIATION THE STATLER HILTON HOTEL, NEW YORK CITY DECEMBER 29, 1968, 4:30 P.M.

The Business Meeting of the Association convened in the Georgian Room of the Statler Hilton Hotel. Approximately two hundred members attended, and President John K. Fairbank presided.

The President opened with a comment on the structure of the AHA. Under the Council and the Nominating Committee, elected by mail ballot of the membership, there are some two hundred committee members who pursue the major interests of the AHA. In general, there are three sources of authority in the Association: first, the elected Council and Nominating Committee; second, the Annual Meeting; and, third, mail ballot of the full membership.

The first item of business was the report of Mr. Paul L. Ward, Executive Secretary of the Association. Since it had been distributed and was available in mimeographed form, he invited questions and comments.

Next Mr. Ward summarized the chief decisions reached by the Council in its three meetings of March 14, September 28, and December 27. Minutes of these meetings appear in the October, December, and April issues of the *Review*, respectively. Among other items, Mr. Ward read the resolution adopted by the Council, two days earlier, ruling against publication at this time of the list of institutions prepared by the Committee on Ph.D. Programs in History. In reporting on the resolution regarding participation of the American Historical Association in the Moscow Congress in 1970, he explained that the AHA's participation in such congresses is formally indirect, since the US national committee that functions under the International Committee need not be, though at present it is, the AHA Committee on International Historical Activities. Mr. Fairbank asked for comment or questions from the floor.

Richard Watson of Duke University, chairman of the Committee on Ph.D. Programs in History, said that his committee had met earlier in the day, with six members present, to consider the actions of the Council. With one member opposed and one abstaining, the committee wished to propose the following motion:

Resolved, That the Business Meeting instruct the Executive Secretary to carry out the Council's authorization of May 1967 and publish, beginning with the next *Newsletter*,

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a list of those institutions meeting the standards for Ph.D. programs in history approved by the Council and published in the *Newsletter* of October 1967.

After Mr. Watson answered questions from the floor, Walter Berg of Central Washington State College, also a member of the committee, urged that publication of the list was essential to help the graduate student and that the profession should police itself during this period when graduate programs were rapidly expanding. Mr. Fairbank, speaking on behalf of the Council, said that the Council was not insensitive to the graduate student's needs, but thought publication of such a list verged on accreditation, which would properly involve much more work than this committee could as yet perform. The Council felt the AHA should move slowly in this direction; efforts should not stop. E. David Cronon of the University of Wisconsin, the new chairman of the Committee on Ph.D. Programs in History, urged the support of Mr. Watson's motion on the grounds that the original purpose of helping institutions improve their programs would otherwise be lost. Howard Cline of the Library of Congress initiated brief discussion of the possibility that the AHA instead issue a booklet of information on doctoral programs similar to that published by the American Anthropological Association.

Mr. Fairbank suggested a mail ballot, but there were many negative voices. The question being called, sixty-four supported the motion, with fifty-two against it. Mr. Watson's resolution was declared adopted.

W. Stull Holt of the University of Washington then moved the following:

Resolved, That the American Historical Association, recognizing the right or even the obligation of individual members to take public stands on current issues, must not commit the historical profession to any position on such issues unless they are directly connected with the promotion of historical scholarship or are necessary to preserve professional integrity; nor should the Association seem to authenticate by lending its name to any interpretation of history whether written, spoken, or pictured.

This motion was put to a vote and adopted.

It was asserted from the floor that this action reflected against the Association's decision to move the 1968 meetings from Chicago. Mr. Fairbank said that the decision to change the location had been a practical and not a political one. A motion was made to reconsider the action just taken. In response to a question, Mr. Holt said that, while the move from Chicago was considered in framing it, his resolution was presented to lay down a principle that should guide future decisions. After further discussion, including a proposal to commend the Council for its decision to move the meeting, the question was called for, and the motion to reconsider Mr. Holt's resolution was lost.

Mr. Robert F. Byrnes of Indiana University next presented the following resolution:

Resolved, That the Association instruct its Committee on International Historical Activities to ask the American representative to the Bureau to work to have the Bureau transfer the Thirteenth International Congress of Historical Sciences in 1970 from Moscow to another acceptable city; and that if the Congress is not transferred from Moscow, the Association, through its Committee on International Historical Activities, should withdraw its official participation from the Moscow Congress.

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Mr. Byrnes urged that it would be indecent and irresponsible to meet in the Soviet Union in view of the latter's invasion of Czechoslovakia. Peaceful relations and scholarly exchange would be best served if the meeting with Soviet historians took place in another country, whereas Americans going to Moscow would give the USSR political capital. Arthur M. Schlesinger reported the decision of the British national committee to withdraw from the Moscow Congress and urged similar action. Boyd C. Shafer of Macalester College, the member of the Bureau referred to in the motion, responded that no one approved or condoned what the Russians have done in Czechoslovakia. His correspondence with some two hundred historians, including many individuals in Britain, indicated predominant preference for continuing with plans to go to Moscow on the grounds that we should not destroy this one bridge between Western and Russian historians. Things could get better, or worse, and the Bureau of CISH was composed of able men in continuous communication with the problem. The AHA in the past has avoided political decisions, he pointed out. After further discussion, a vote was taken by show of hands, and the motion was declared lost.

Richard Wade of the University of Chicago next moved that a mail ballot of the AHA membership be taken on the issue, after careful statements from both sides are given. After brief discussion, this motion carried.

The Managing Editor, Mr. Webb, then read his report, which was duly accepted.

Mr. Fairbank prefaced the Treasurer's report by reading a resolution of gratitude that had been adopted by the Council. The Treasurer directed attention to the written report submitted and passed out to all attending the Business Meeting. In view of the lateness of the hour, he did not add comments. The Treasurer's report was approved.

Since there was no old business, the next item on the agenda was new business. Harold C. Deutsch of the University of Minnesota suggested a resolution deploring the severe mutilation of the Fulbright program. Mr. Ward reported that the Council had adopted the following resolution to a similar effect:

> *Resolved,* That the American Historical Association views with deep concern recent cuts by the federal government in the Fulbright program and other similar programs of international scholarly exchange and cooperation. It strongly urges that the new administration and Congress restore such cuts in the interest of the United States' role of leadership in the world.

Mr. Deutsch accepted this wording, and a resolution identical with that of the Council was then moved, seconded, and passed.

Mr. Ward asked the members to vote on three proposed constitutional amendments that had been reported in the *AHA Newsletter* (VII [Oct. 1968], 2).

The first was an amendment to Article VI, Section 1, of the Constitution, which would be changed to read as follows:

> SECTION 1. There shall be a Nominating Committee to consist of seven members, each of whom shall serve a term of three years. In successive years, the new members shall be elected as follows: three the first year, two the second year, and two the third year; this alternation shall continue except in the case of elections to complete unex-

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pired terms. If vacancies on the Nominating Committee occur between the Annual Elections, the Nominating Committee shall fill them by direct *ad interim* appointment.

It was moved and voted to adopt this amendment.

Mr. Ward next read the second amendment regarding a change in dates for return of ballots, from December 20 to December 15. The relevant sentence of Article VI, Section 2, of the Constitution as amended would read:

> The members of the Association shall take their choice from among these nominations and return their ballots for counting not later than the fifteenth of December at 6:00 p.m.

It was moved and voted to adopt this amendment.

The third amendment proposed by the Council is that Article III, Section 1, of the AHA Constitution be amended to read:

> SECTION 1. Any person approved by the Council may become an active member of the Association. Types of memberships, the amount of dues, and the date upon which any change of dues becomes effective shall be fixed by the Council, after due notice to the membership. Life membership shall be given members who have belonged to the Association for fifty years. Annual dues shall be payable at the beginning of the year to which they apply and any member whose dues are in arrears for one year may, one month after the mailing of the notice of such delinquency to his last known address, be dropped from the rolls by vote of the Council or the Executive Committee. Members who have been so dropped may be reinstated at any time by the payment of one year's dues in advance. Only active members shall have the right to vote or to hold office in the Association. Persons not resident in the United States may be elected by the Council as honorary or corresponding members, and such members shall be exempt from payment of dues.

This amendment was also duly adopted.

Next on the agenda was the report of the Nominating Committee. Bryce Lyon, the chairman, announced that Peter Gay, Columbia University, and Felix Gilbert, Institute for Advanced Study, were elected to the Council. C. Warren Hollister, University of California, Santa Barbara, David Shannon, Rutgers University, and Thomas McGann, University of Texas, were elected to the Nominating Committee. By a motion made and seconded, this report was approved as presented. On behalf of the committee, Mr. Lyon next placed the following names in nomination as officers of the American Historical Association for 1969:

President	C. Vann Woodward
Vice-President	Robert R. Palmer
Treasurer	Elmer Louis Kayser

Noting that no further nominations had been received, he moved the election of these nominees. The motion carried unanimously.

Caroline Robbins presented two resolutions, the second of which was amended by suggestion from the floor, as follows:

> *Resolved,* That the AHA offers its thanks to its 1968 Program Committee, under the chairmanship of Lawrence W. Towner, for the efficiency and originality that it displayed in arranging this year's meetings.

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Resolved, That the AHA particularly thank the Committee on Local Arrangements, under the chairmanship of Robert W. Shoemaker, and with the able assistance of Douglas W. Houston as cochairman, for its ingenuity in providing so swiftly for the complex details involved in moving the meeting, originally planned for Chicago, to new quarters in New York.

It was moved and seconded to adopt these resolutions, and the motion carried.

Miss Robbins next read a copy of a resolution to be sent to the office of the President-elect, Richard M. Nixon, concerning the appointment for the position of Archivist of the United States:

WHEREAS, The position of the Archivist of the United States has always to date been a nonpolitical appointment; and

WHEREAS, The present Archivist, Dr. James Berton Rhoads, came to this office last May with a strong background of experience in archives' administration and scholarly competence as a historian,

Resolved, That, at this time of change of presidential administration, the American Historical Association place on record its conviction that the Archivist of the United States should remain a nonpolitical appointment depending wholly upon professional and scholarly competence.

It was moved, seconded, and carried that this resolution be adopted.

Since there was no further business, the meeting was, upon motion, adjourned at 6:54 p.m.

PAUL L. WARD, *Executive Secretary*

[The following letter, dated January 23, 1969, was received by the Executive Secretary as a supplement to the minutes of the Business Meeting.]

The annual business session, on December 29, 1968, had enough problems to discuss, and as chairman I preferred not to precipitate a time-consuming post-mortem on the decision last September to move the December meeting of the AHA from Chicago to New York. For the record, however, certain points should be clarified.

1) The elected officers and other Council members of the AHA represent an incorporated legal individual, but the AHA is not a political animal nor has it a moral personality. Its interests are professional and academic, to assist teaching and scholarship by individuals. What such individuals may do in politics is not its concern.
2) Its one "political" concern is to help maintain a context of freedom for research, teaching, and scholarly expression; several AHA committees devote themselves to various aspects of this ramified problem. If an overwhelming consensus of well-informed members had felt that scholarly freedom would be threatened by our remaining in Chicago, we would no doubt have had a valid "political" reason (in the above sense of concern for the essential context of scholarship) for leaving Chicago. But such did not seem to be the case. The events of last August in Chicago were from the first highly controversial. There was no overwhelming consensus.
3) Why then did we leave Chicago? The answer is simply—to ensure the success of the AHA's annual meeting. This is a chief practical responsibility of the officers, the other Council members, and several principal committees each year. The success of the annual meeting, if held in Chicago, was threatened first by the personal feelings of a considerable body of AHA members who from all parts of the country urged us to

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leave Chicago and in many cases explicitly stated they would not come to Chicago. (Within the first four weeks, we received representations against meeting in Chicago from roughly eleven hundred AHA members and from only about forty in favor.) Predominant majorities of a number of major departments, of the Pacific Coast Branch annual business meeting, and of the Program Committee, all were against meeting in Chicago, to a point where we had every reason to believe attendance would be seriously reduced and the program itself seriously impaired.

Second, we believed the student feeling about Chicago was such that we could expect a meeting there to be further impaired by demonstrations if not actual disorders or, alternatively, by their active suppression. I for one concluded that the interests of the AHA would be best served by meeting elsewhere. (As it turned out, our fortunate arrangement with the Statler Hilton in New York, since it was part of the same corporation, saved us in a legal sense from technical breach of contract with the Conrad Hilton in Chicago. The latter in any case did not contemplate legal action, and our relations remain cordial for the future.)

I am aware that such decisions in a time of controversy are given political significance as symbolic gestures. Some told us that to leave Chicago would be to put the AHA into politics. Others told us that not to leave would equally be a political gesture.

In my opinion both these views are at fault for 1) understressing the practical interest of the AHA in having a successful annual meeting and 2) imputing political motives for what this nonpolitical association does in pursuit of its professional work. Both these views attempt to *politicize* AHA decisions, to put political considerations, either pro or con, ahead of scholarly, professional considerations. Such a tendency from either side strikes at the pluralism of our institutions.

I initiated the Chicago decision as a practical, administrative matter, for the health of the annual meeting. If a thousand AHA members had opposed going to Chicago for some other reason, say because of flouridated water, the decision could have been the same and on the same grounds.

4) The anti-Chicago AHA members were no doubt voicing their personal political views, and many if not most would no doubt claim that they felt a legitimate "political" concern in the sense I have noted above (to defend free institutions that nurture history, etc.). Pro-Chicago AHA members may with equal propriety have felt the same concern in support of their view. *However,* and this is the point, the officers and other members of the Council were not therefore called upon to express their personal political views. They were in a different position, with more definite responsibilities, confronting a decision which because of the late date could not be delayed. They were called upon not to make political gestures but to pursue the best interests of the Association. This they did, and I submit that the results justified their action.

JOHN K. FAIRBANK

RECENT DEATHS

John Sydor of Riva, Missouri, died July 31, 1968.

Sister M. Alphonsine Frawley, C.S.J., chairman of the department at Regis College, died August 6.

Egbert R. Isbell, professor emeritus at Eastern Michigan University, died October 8.

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David Dowd, a professor at the University of Kentucky, died October 25, at the age of fifty. He was chiefly interested in the French Revolution and Napoleon, and his publications included *Pageant-Master of the Republic: Jacques-Louis David and the French Revolution* and *Napoleon.*

James J. Hannah, associate professor at the University of Santa Clara, died December 9.

Fred M. Hamil, who recently retired from the department at Wayne State University, died December 24.

In a long, active, and fruitful life that ended on December 26, Kenneth Scott Latourette (President of the American Historical Association in 1948) combined at least three successful careers—as an evangelical Christian, as a historian of China, and as a historian of the expansion of Christianity. He was born in Oregon City of parents who were devout Baptists, and he lived his life in the service of Christian faith, careful scholarship, and good works. The scope of his Christian interests steadily broadened, from Sunday school, Bible classes, and the YMCA to the Student Volunteer Movement for Foreign Missions and eventually the ecumenical movement in general. He helped set up the World Council of Churches in 1938, was active for many years in the International Missionary Council, and meanwhile served on the boards of Yale-in-China, Oberlin-in-China, Nanking Theological Seminary, the International YMCA, the United Board for the Christian Colleges in China, and many other institutions. In 1951–1952 he was president of the American Baptist Convention. Such posts involved much sustained activity, in Christian fellowship with a multitude of warm friends and coworkers around the world.

After obtaining a B.S. from Linfield College at McMinnville, Oregon, in 1904, Latourette transferred to Yale. He went into history almost casually, yet it proved to be the major outlet for his intellectual talent. He graduated in the class of 1906 already determined to become a missionary teacher at Yale-in-China in Changsha. But in preparation for this career he took a Ph.D. in history in 1909, studying under George Burton Adams, Edward Gaylord Bourne, and K. Asakawa and writing his dissertation on early Sino-American relations to 1844 under Frederick Wells Williams. He reached Changsha in 1910 and studied Chinese, but dysentery forced him to return to Oregon to recuperate in 1912. There he began to teach history, both of China and of Christian missions, first at Reed, then at Denison, and finally at Yale—from 1921 as professor of missions in the Divinity School, from 1927 as professor also of Oriental history in the history department, and eventually as Sterling Professor in both subjects. Here he found his calling. A bachelor and a systematic and unremitting worker, self-trained to write two thousand words a morning, rain or shine, he produced in all some ninety separate volumes of which at least forty were substantial hard-cover books, mostly in history. Best known and valued in their fields were his balanced summary, *The Chinese: Their History and Culture* (2 vols., 1934; 3d rev. ed., 1964) and his pathbreaking, comprehensive work, *A History of the Expansion of Christianity*

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(7 vols., 1937–45). But there was a host of other volumes, some chronicling a new subject like the YMCA World Service, others written for different audiences to summarize narratives already worked out.

Latourette was a scientific historian of the prerelativistic school, a scholar who thoroughly combed the literature, amassed the factual record, and organized it in serviceable prose that was clear, judicious, and unimpassioned. His scope expanded with his work, from *A History of Christian Missions in China* (1929) to missions everywhere, both Protestant and Roman Catholic, and a concern for the ecumenical unity of the church throughout the world. In view of this major interest, it is all the more remarkable that he should have been also the major historian to introduce several generations of American students to the whole range of Chinese civilization. After his retirement at Yale in 1953, he remained an active fellow of Berkeley College, and in 1954–1955 he served as president of the Far Eastern Association (now the Association for Asian Studies). The five volumes of *Christianity in a Revolutionary Age,* that is, in the nineteenth and twentieth centuries, were published in 1958–1962. Holding to his own faith so firmly, Latourette was a man of broad tolerance and eternal optimism; his work was generally commended for its impartiality. Few historians have instructed so many about distant countries or laid so solid a factual foundation in a newly developing field.

Chandler Bragdon, professor emeritus at the State University College of Arts and Science, Plattsburgh, New York, died January 3, 1969.

Members of the Association who have died recently include: George Douglas Blackwood of Arlington, Massachusetts; J. Claude Roberts of Stephen F. Austin State College; Leo Stoller of Wayne State University; Lois B. Turner of Kansas State University; Ira Jewell Williams of Philadelphia, Pennsylvania.

Communications

To the Editor of the American Historical Review:

With reference to Mr. Bausum's unfavorable review of my *Further Selections from the Tragic History of the Sea, 1559–1565* (*AHR*, LXXIV [Oct. 1968], 193), my criticisms of Gomes de Brito's editorial deficiencies were not made "cavalierly," but as the result of careful consideration, and they are supported by strong arguments. I might have been still more critical of Gomes de Brito's failings, but for my realization that one should not be too hard on the Lisbon equivalent of a Grub Street hack. The fact remains that Gomes de Brito has misled all bibliographers concerned with the *Historia trágico-marítima* for over two centuries, and it is high time that his shortcomings were exposed.

Far from "leaving unexplored" the possibility that Gomes de Brito might have based some of his narratives on editions that are now missing, I have carried this process further than any other writer on the subject, by proving (pp. 19, 160) that the missing first edition of Jorge d'Albuquerque's *Naufrágio* must have been

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published at some date between about 1580 and 1592. Readers who fancy that there might be something in Mr. Bausum's criticisms may be referred to other reviews of my book.

Indiana University C. R. Boxer

To the Editor of the American Historical Review:

I would not question Mr. Boxer's maligning of Gomes de Brito as "the Lisbon equivalent of a Grub Street hack" except that he does not substantiate his rhetorical attack on Gomes de Brito with sufficient evidence. In the case of the Dias narrative, he concedes that Gomes de Brito used an early edition of Dias (p. 159), an edition now lost to scholars. My issue with him is that he flatly accuses Gomes de Brito of inserting changes without having seen the original source. In the case of the Do Couto and *Naufrágio* narratives, Mr. Boxer assumes that both he and Gomes de Brito consulted the same editions (pp. 2, 160). But, without really examining the validity of his assumption, Mr. Boxer charges Gomes de Brito with "arbitrarily suppressed" matter.

In effect, Mr. Boxer allows his arguments to obfuscate his logic both in the book itself (note his simplistic notion of proof, p. 160) and in his letter. Proving the existence of the "1580–1592" edition of the *Naufrágio,* for example, does not also substantiate that Gomes de Brito might have used it. Has Mr. Boxer explored this possibility? Where, in fact, are his "strong arguments"? If asking for concrete evidence that Gomes de Brito was a hack, which he may very well have been, is "captious criticism," I offer my apologies to Mr. Boxer.

Virginia Military Institute Henry S. Bausum

To the Editor of the American Historical Review:

I would like to make a correction in my recent review of Thomas M. Barker's *Double Eagle and Crescent: Vienna's Second Turkish Siege and Its Historical Setting* (*AHR*, LXXIV [Oct. 1968], 208), and I would be obliged if you published it.

At the time I stated that this excellent book could have been further enhanced by use of documentary materials in the Vienna War Archive, especially in the *Feldakten*. However, after working again in the archives, I am now able to corroborate the negative outcome of Mr. Barker's own search there. Although, theoretically speaking, manuscripts concerning this topic may lie forgotten among private papers or mislaid in public repositories, the possibility of discovering significantly new material in European sources does not appear great.

I have communicated the substance of this letter to Mr. Barker and think that it should be told to the profession.

University of New Mexico Gunther E. Rothenberg

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To the Editor of the American Historical Review:

I read with interest Mr. Roderic H. Davison's review of my *Cyprus: Conflict and Conciliation, 1954–1958* (*AHR,* LXXIV [Dec. 1968], 667). I want to make clear that the "sarcastic remarks on British moves" that your reviewer mentions are not my own, but are taken from the documents used in the preparation of my book. If I included them in my text, it was to illustrate the attitude of members of a small group in reporting about the activities of their opponents in a dispute. Since I was never a member of the Greek delegation at the UN General Assembly, nor was I present during the airing of the Cyprus dispute or the relevant backstage activities, these attitudes could hardly be my own.

As for Mr. Davison's remarks that the extensive summaries of speeches in the First Committee of the UN General Assembly are boring, this is so because these debates *are* boring to parties not involved directly in an issue, and they eventually become boring even to them. However, if these summaries could not be cut down further or excluded, this was because I wanted the reader to listen to the arguments of the parties directly concerned and to become acquainted with the views of third parties to the dispute (the speeches of the latter being far more summarized than the speeches of the former), even at the risk of boring them. This book was not written for purposes of entertainment but for instruction in the process of "parliamentary diplomacy" and its ramifications, as a careful reading of my conclusions might have helped Mr. Davison to understand.

Hunter College Stephen G. Xydis

To the Editor of the American Historical Review:

Kindly allow me to comment upon the review of my work, *The Thin Disguise,* by Leslie H. Fishel, jr. (*AHR,* LXXIV [Dec. 1968], 739). I have no desire to quarrel here with Mr. Fishel's theories or with his apparent indignation over my own indignant response to the traditions of racism and Jim Crow, but I must protest that his review obscures my treatment of those matters.

According to Mr. Fishel, my "message does come through: an accelerating and overriding belief by whites in the incapacity of the Negro, and their determination to write the incapacities into law." Actually *The Thin Disguise* presents no such simplistic view of either the nature of prejudice or the evolution of Jim Crow. It does assert that demands for legally imposed segregation were widespread only in the South; that the North was characterized by general disinterest and signs of more opposition than support for such segregation; and that the Plessy decision of 1896 reflected historical circumstances, the convenience of the establishment, and the materialistic, elitist business mentality of the late nineteenth century. The volume states that whatever the growth of segregationist sentiment "it does not follow, however, that the Plessy decision was forced upon the Supreme Court by overwhelming popular pressure, nor should one expect such to be the case for a court so noted for frustrating the common will. Rather,

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in reacting to a variety of forces acting for and against racial distinctions in law, the court was guided by its own aristocratic predilections toward a crucial endorsement of racial segregation as an American way of life. Its decision stimulated the rapid and pervasive spread of enforced segregation and, like so many of the court's earlier pronouncements, contributed to a general intensification of racist oppression in the United States."

Northern Illinois University OTTO H. OLSEN

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Yankee Stepfather

General O. O. Howard and the Freedmen

by William S. McFeely

This biographical study of O. O. Howard provides a new understanding of the historical relationship between black Americans and the federal government by discussing what Howard hoped to accomplish as commissioner of the Freedmen's Bureau. His hopes and plans for the freedmen are considered in light of both the aspirations of the freed slaves and the reluctance of Andrew Johnson's white America to allow them to be reached. $10.00

Strange Enthusiasm

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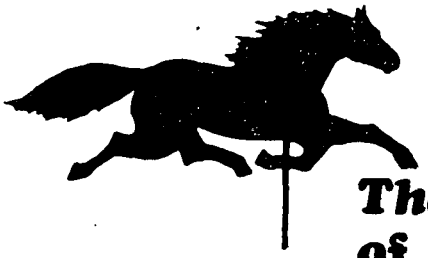
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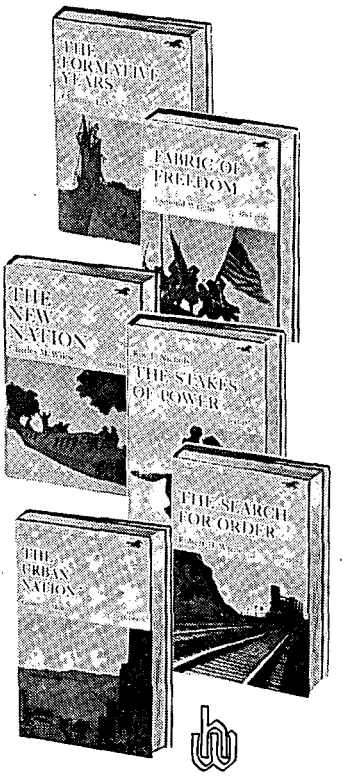
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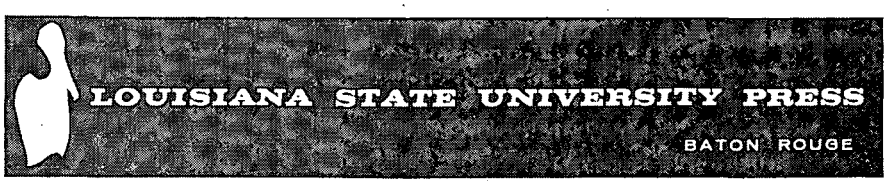
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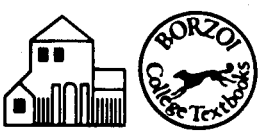
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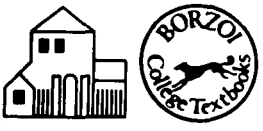
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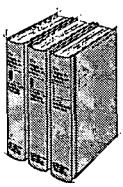


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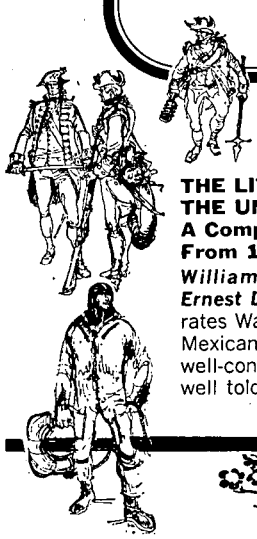
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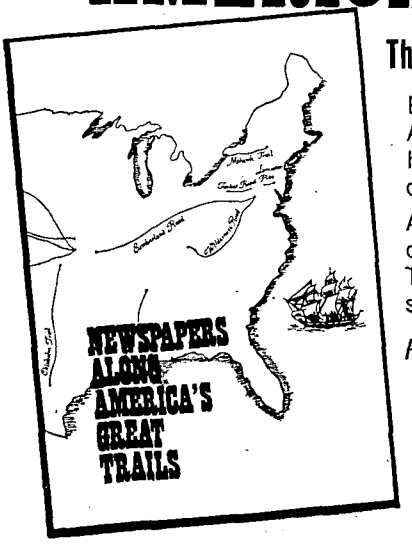
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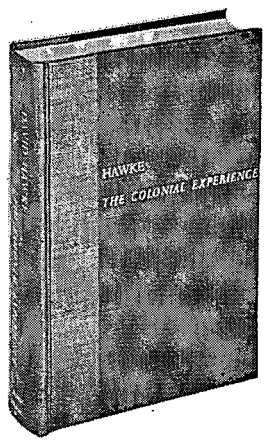
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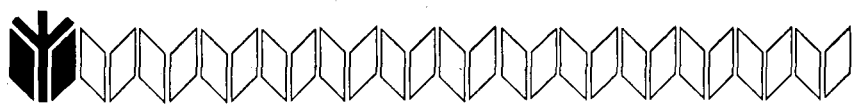
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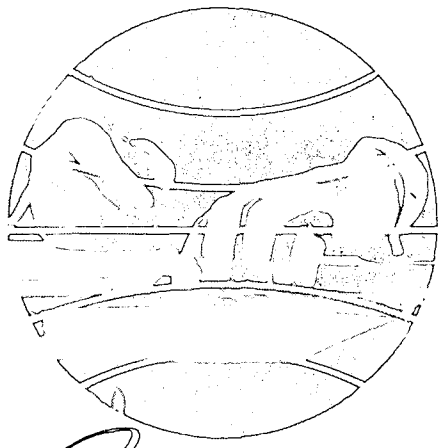
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